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AGRICULTURAL POTENTIAL AND THE DEVELOPMENT OF CULTURES*

EDWIN N. FERDON JR.

SEVERAL YEARS AGO an attempt was made to classify environments in terms of their agricultural potential, subsistence being regarded as that phase of culture where the greatest degree of culture-environment interaction takes place and hence the most critical for the study of that phenomenon.[1] The idea not only is basically sound but has theoretical as well as practical applications, especially in today's under-developed regions of the world. Unfortunately, the resultant classification was inadequate in that it was a culture-environment classification and, therefore, its use in attempting to prove environmental control over cultural development has been unconvincing. The present paper is an attempt to classify natural agricultural potential on the basis of observed and measurable environmental elements each of which is classified on the basis of its apparent annual growth-inducing characteristics. The various combinations of the classified elements are then grouped into all possible combinations and the resultant environmental complexes rated as to their agricultural potential, the basis being an idealized concept of minimum farming techniques, that is, the least cultural interference with natural growth. The resultant natural agricultural potential ratings are then applied to the problem of whether or not natural environment controls cultural development through the presence or absence of ideal natural conditions for agricultural subsistence.

* I want to thank Bruce Ellis, Alfred E. Dittert, Jr., and Stewart Peckham for their critical reading of this manuscript.

1 Betty Meggers, *Environmental Limitation on the Development of Culture* (American Anthropologist, vol. 56, pp. 801-823, 1954).

Before taking up our own approach to the problem, it would be advantageous to briefly review Meggers' four-fold classification and indicate what we regard as its critical weaknesses.[2]

Type 1. Areas of no agricultural potential. This is said to include a variety of landscapes, the critical factor being that "only one of the many components necessary for agriculture need be absent for the area to be unsuitable." Type 1 regions are seen as including tundra, some deserts, tropical savannas, swamps, some mountain ranges, and similarly uncultivable land.

Type 2. Areas of limited agricultural potential. Here "limited soil fertility, which cannot economically be improved or conserved" is the critical factor. The soil leaching effect of high precipitation and humidity is said to point to this type's being tropical in location, although it is admitted that not all tropical environments are of this type.

Type 3. Areas of increasable (improvable) agricultural potential. This type is said to pertain to areas with temperate climates where rainfall and humidity are less detrimental to soils, and soil exhaustion is caused mainly by the raising of crops. Under a slash and burn agricultural technique this type is thought to produce only slightly more food than under a similar technique in a Type 2 environment. However, under rotation, fallowing, and fertilizing Type 3 environment is viewed as being capable of almost continuous food crop production over long periods of time. Since present agricultural techniques, where they have been applied to specific areas, are also included in setting up this classification, temperate desert areas, such as the Imperial Valley of California, may be included in Type 3 environments, for although it is naturally deficient in moisture, irrigation techniques have counterbalanced this deficiency.

Type 4. Areas of unlimited agricultural potential. "Here the natural environment approximates as closely as possible the ideal conditions for agriculture," and ". . . the land can support intensive food production indefinitely. The *'cradles of civilization' all belong to Type 4.*"

The method of arriving at these environmental ratings appears inconsistent and confusing since, for example, deserts are included in the lowest, or Type 1, environments and again in the next to best, or Type 3, environments. Here the difference between the two deserts is stated to be the presence of irrigation in the latter and its absence in the former. The difference, then, is not based on the natural environment but on culturally introduced factors. Furthermore, the reason the undeveloped desert does not have the same potential rating as the developed

2 *Idem*, pp. 803-805.

is based on the fact that the author arbitrarily has placed a base-line date of 1950 for the rating of all environments. Thus, if a given environment had not been greatly improved agriculturally as of that date, it was regarded as unimprovable. Such an approach is hardly valid in rating the agricultural potential of any area, for in numerous cases the key factors controlling the presence or absence of development are political or economic and do not necessarily reflect a technological inability to cope with a particular environmental situation. Where the confusion occurs is in the lack of realization that agricultural potential, as such, is a cultural appraisal based upon available knowledge and techniques of what can be used, or manipulated to advantage, in the natural environment of a particular region. Economics, politics, or lack of cultural need or desire may withhold the development of a particular potential, but the potential remains unaltered. This confusion as to what is being attempted is developed most clearly in the primary descriptions of certain of the environmental types listed above. Thus, Type 1 is stated to have "no agricultural potential," and Type 3 is rated as having "improvable agricultural potential." If there is one thing clear in agricultural science it is that all areas of the world, especially the poorer ones, are capable of improvement, and no region totally lacks agricultural potential. Agriculture is a technique applicable to tundra mosses as well as to maize or millet. After all, artificial soils have been produced on soilless atolls in Polynesia, steep mountains have been terraced for agriculture, deserts have been made to blossom like the proverbial rose, and in one extreme case a small garden of vegetable greens has been grown on an ice flow.[3] In other words, the techniques for such production are known; whether or not they have been employed as of any date line is not a matter of present ability but lack of need or economic feasibility at the moment. The fact that people may be starving in China, India, or elsewhere does not mean that all of the agriculturally usable lands of the world are at peak production. If anything, the reverse is true.

AGRICULTURAL ENVIRONMENTS CLASSIFICATION

In studying the relationship of natural environments to the culturally derived technique of agriculture, we are dealing with that segment of total environment which directly influences plant growth. Since agriculture is an artificial means of increasing the growth and yield of a given plant, or plants, over, or at the expense of, other plants, and the culturally derived knowledge and techniques employed in such activity varies from the extremely simple to the highly complex, it is necessary to establish a technological base level upon which to judge the potential of

3 *The Coldest Cold War* (Newsweek, vol. 44, no. 20, pp. 54-56, 1954), p. 54.

any given environmental complex. By establishing this base at an idealized minimum level, conceived of as amounting to simple clearing and planting, our potential ratings may be judged on the basis of minimum cultural interference and maximum environmental control. Thus, the greater the agricultural yield with the least employment of special agricultural techniques, the higher should be the natural agricultural potential rating, for more people of varying technological development can use the environment with success. The more difficult the natural situation, the lower the potential rating should be, for more botanical knowledge of processes and a greater development of techniques will be needed to practice successful agriculture in such a locality. No date line is needed in this approach to a classification, for it is here taken as a premise that present agricultural knowledge is sufficiently advanced, or can be made so if necessary, to maintain agricultural production on any land mass in the world, given the availability of presently known sources of energy and sufficient economic and subsistence pressure to force the issue.

Admittedly, any attempt to classify those elements of the environment having to do with agriculture is, at best, only a poor approximation to actual conditions. Not only are there a multiplicity of natural factors in the growth of any plant, the optimum requirements differing from plant to plant, but even a slight variation in agricultural technique may set off a chain reaction which can result in greater, or lesser, production than before. A further hindrance is the general lack of knowledge of the less obvious but important environmental factors for large parts of the world. However, to await the total accumulation of all pertinent data is to stand still. There is, after all, a certain amount of basic information pertinent to the study and classification of natural agricultural potential which may be used as a preliminary approach to this highly complex subject. The principal natural factors concerned with agriculture, which can be broken down into numerous sub-divisions, are temperature, precipitation, soil, and terrain or topography. In the first two it is not enough to know the average yearly figure since the annual regime of temperature and rainfall is equally important. It is the length of the growing season and its relation to available moisture that is critical in the growth potential of a plant. Although not universally true, a reasonably friable soil with high organic content has proved the best type for at least the grain foods while, except for such crops as wet rice and taro, or where mechanized farming is practiced, terrain ranging from flat plains to rolling mountains has been made to yield plentiful crops under hand or ox-plow cultivation.

Recognizing, as does Meggers,[4] that the lack of any one of a fairly numerous

4 Meggers, *op. cit.*, p. 803.

series of components in an agricultural environment may make that environment unsuited to agricultural, we are forced, for the time being, to neglect some of these for lack of broad-scale knowledge of their distribution and effects under differing environmental and agricultural situations. There are, however, certain obvious gross components which can be used on a broad basis. These are lack of soil, or usable soil, that is, ice cap, permanently frozen soil, bare rock; lack of any definable growing season due to low temperatures, low precipitation effectiveness, or both; or the presence of a terrain so steep and rugged that no agriculture is feasible without artificial terracing. These three extremes may, for the purposes of this report, be regarded as absolute deterrents to agriculture where nothing more than clearing and planting is employed. They are not absolute deterrents, however, where culturally derived techniques have been sufficiently developed.

At the other extreme of the environmental element rating scale, that is, the optimum, we must include a growing season of 365 days a year, a precipitation pattern adequate for plant growth throughout the year, a soil type having fair to high organic and mineral content, and a terrain type ranging from level plains to rolling hill country and plateaus and basins. Productive agriculture is known to obtain in certain parts of the world on terrains outside of these categories, but they are more often than not special situations which cannot be included in a broad classification.

With the extremes of our element classification outlined above we arrive at the far more difficult problem of determining the middle ratings. An immediate break-down of temperature and rainfall can be made on the basis of their effect upon the length of a growing season. From the point of view of temperature, and the length of the growing season, the tropics, below a varying limit of altitude, present the possibilities of a 365 day growing season thus allowing more than one crop a year from the same plot of land. The mid-latitude temperate climates present an average of a six month period of plant growth and, in parts of the microthermal climates, a maximum of one extremely short growth period. Whereas temperatures may be adequate for plant growth throughout the year, the factor of precipitation pattern and effectiveness may enter to nullify the advantages of the temperature factor. Thus, in tropical savannas, or tropical Cw climates,[5] rainfall is adequate for only about six months of each year and under natural conditions plant growth is possible only during the periods of adequate moisture. Culturally this periodicity, or lack of periodicity, becomes vitally important. Where temperature and moisture are adequate the year around, continuous crop production is a distinct possibility, if soil and terrain are adequate. On the other hand, where

5 The Köppen system for designating climatic types is used throughout the text.

agriculture is possible only part of the year, owing to deficient temperature or moisture, an average of only one crop a year can be obtained from any given parcel of land under a simple clearing and planting situation. Therefore, only half the number of mouths can be fed from a given field.

It is in the factor of soil that I, for one, find the greatest difficulty in attempting to rate the middle types between the two extremes. Much of this difficulty stems from the very youthfulness of the science and the present impossibility of arriving at any adequate formula for the determination of potential yield for a given soil under a given rainfall and temperature situation. The critical factor here appears to be the agricultural technique employed in growing the particular crop. So important is this factor that the United States Department of Agriculture determines yield only on the basis of actual planted crops, and the resultant yield is described in terms of the agricultural methods employed.[6]

Difficult as the soil situation appears to be, especially when dealing with small areas of diverse soil types, we can, when viewing agriculture on a broad time and space perspective, note that at least five major soils are represented in those areas of the world which have produced or are producing the major food crops of the world. These are the chernozem and the prairie soils, the podzolic soils of Europe, the desert grey and brown soils of the desert and semi-desert regions, and the rich silt deposits of the flood plains of such rivers as the Nile, Mississippi, and Irrawaddy. Because of the great depth of the organic material in the chernozem and the prairie soils, which nearly defy exhaustion from long continued cropping, and the annual renewal of soil fertility through flooding of the flood plains of many of the great rivers, these three soils stand out as probably having the highest natural soil potential for most, if not all, of the grain crops of the world. Next in importance appears to be the podzolic soils, and at the bottom of the list are the red and yellow leached soils, lateritics and podzols.

With the above in mind we have developed the following environmental ratings in which the basic factors important to agriculture, i.e. temperature, moisture, soil, and terrain, are broken down into four arbitrary divisions ranging from highest to lowest apparent natural crop growth potential. In their construction we have drawn upon the knowledge of the climatologist, the soil scientist, and the geographer. In the realm of climatology we have used the well-known and easily manageable Köppen system. For soils we have virtually been forced to use the genetic system owing to the fact that this system, as far as can be ascertained, has

6 *Soil Survey Manual* (United States Department of Agriculture Handbook no. 18, 1951), p. 367.

been used to a greater extent than any other in plotting world distributions of major soil types. For terrain classification we have turned to the geographers.

In using this element classification on a broad scale it must be clearly understood that it represents a gross rating picture, primarily of the grain crops. In other words, it is impossible to give a precise rating since each plant has its own specific environmental requirements and, to further complicate things, its reactions change under slightly differing agricultural methods. However, the gross classification of the various environmental factors, as employed here, is thought to approach a true representation of the approximate range of significant evaluations for the culturally more important food crops of the world when produced under the simplest agricultural methods. For more specific and detailed studies the following system of factor ratings can, if desirable, be used for individual crops, such as maize, beans, or wheat, and even expanded for the recording of more detail by the addition of letters or numbers. In this way it can become useful in recording and rating the more detailed information required in modern scientific farming, especially where planned agricultural development is contemplated.

ENVIRONMENTAL FACTOR RATINGS[7]

Temperature

A. Growth-inducing throughout the year.

The temperature elements in Köppen's Af, Aw, BSh, BWh, and highland tropical C climates fall within this category.

B. Growth-inducing for about half the year.

The temperature patterns associated with Köppen's temperate C and Da climates as well as his BSk and BWk climates below the approximate latitude of 45 degrees.

C. Sufficient for only a short growing period.

This includes the temperature patterns associated with Köppen's Db climate and his BSk and BWK climates above the latitude of 45 degrees as well as the lower latitude extensions of his Dc climate.

D. Insufficient throughout the year.

Köppen's ET, EF, and Dd climates as well as the higher latitude areas of his Dc climate are included in this category.

7 In regard to the following classification it should be pointed out that for the study of limited areas where large scale maps can be employed it would be more meaningful to record and classify such data as, for example, the number of frost free days, the amount, periodicity, and type of precipitation, and the degree of percent of terrain slope. Also, the United States Department of Agriculture's binomial soil classification would be preferable owing to its greater detail and finer precision in recording soil differences.

Precipitation

A. Adequate throughout the year.

Köppen's Af, Cf, Df, and E climates have the precipitation patterns which place them in this category.

B. Adequate for about half the year.

Köppen's Am, Aw, Cw, Cs, and Dw climates have a periodic rainfall pattern which places them in this category.

C. Adequate for highly limited production.

Köppen's BS climate, except for its desert margins, may be used for dry farming, though unreliable.

D. Inadequate throughout the year.

Köppen's BW, or desert, climate and the drier portions of his BS climate fall within this category.

Soils

A. Good.

Prairie, chernozem, brown steppe, and alluvial flood plain and delta soils.

B. Fair.

Podzolic gray-brown forest soils, desert soils, and rendzina and *terra rossa* soils.

C. Poor.

Laterites and lateritic red and yellow soils, podzols, marshes and gley and tundra soils. Also, porous sand and ash and such unconsolidated soils as youthful gravels.

D. No Soil.

Ice cap, rock, and perma-frost soil.

Land Forms (Terrain)

A. Good.

Plains and low, rolling hills.

B. Fair.

Dissected plateaus and basins.

C. Poor.

Hills and low mountains.

D. Largely inadequate.

Rough mountain terrain.

In recording and plotting the various combinations of ratings of the four basic factors which, when presented together, give the gross total environmental picture at a glance, I have found it convenient to record them from left to right in their

order of occurrence from top to bottom in nature. Thus the first factor letter is the temperature rating, the second moisture, third soil, and fourth terrain. A hilly, tropical savanna with lateritic soil (C) is thus recorded as A (growth inducing temperature throughout the year), B (adequate rainfall only half the year), C (hilly terrain), or ABCC. A frost-free deltaic plain with ample rainfall throughout the year would be recorded as AAAA, the optimum situation for maximum natural agricultural potential per unit of land.

Although the theoretical optimum growth potential for a plant presumes an optimum complex of environmental elements related to its growth, this does not mean that the plant ceases to grow if one or more of the elements do not meet its optimum requirements. The plant may not, however, grow in the same abundance that it would under optimum conditions, nor would it be expected to yield the same amount of fruit. It is determining this degree of difference in potential growth and yield of a plant under varying complexes of environmental elements that is the goal of our natural agricultural potential study. However, it should be pointed out that in practical, applied potential ratings the person evaluating the situation customarily takes into account not only the condition of the natural elements but his culturally derived technical knowledge of what can be done artificially to improve any natural deficiencies which may affect the crop being contemplated. Even more, he judges the use of these techniques on the basis of their economic feasibility or the human need to introduce them in spite of cost. Such an approach to the study of agricultural potential not only is wholly justifiable, but it is the only approach that a practically minded culture can use. Any environmental potential is a cultural appraisal of what the particular culture's knowledge and technical ability can make use of in the environment as judged within the framework of its economic and social organization, its needs, and its desires. However, to use such an approach with the goal in mind of applying the results to prove or disprove environmental control over the totality of cultural development is misleading. In truth, it is a cultural-environmental potential rating that has been made rather than a purely environmental appraisal.

Since we are attempting to determine if environment, through its control of plant growth, can induce or retard cultural development, we have set up our agricultural potential ratings on the basis of the technologically simplest agricultural methods which, presumably, would least interfere with the natural working of environmetal elements upon the chosen crop. This represents the closest that one can approach a purely environmentally controlled agricultural potential rating, since agriculture, as opposed to gathering, is a cultural means of manipulating natural environment to produce more of a certain plant than nature would normally

supply. Thus, although the most simple agricultural technique presupposes a cultural interference with nature's processes, the possible minor effects of such interferences have not been considered in setting up our potential ratings. With the above in mind, we have grouped the 256 possible environmental combinations, as previously classified, into five major grades of natural agricultural potential.

EXCELLENT

Ia Temperature, precipitation, and soil are A and terrain is B or better.

Temperature, precipitation, and soil are excellent for plant growth throughout the year, thus allowing for the greatest possible annual crop yield per unit of land. The range of terrain type is not considered to be great enough to noticeably affect production.

Ib Temperature and precipitation are A, soil is B, and terrain is B or better.

Temperature and precipitation are excellent for year-around plant growth thus allowing continued agriculture. Since soil is of the B, or fair, variety, expected yield per unit of land must be regarded as less than Ia. The range of terrain type is not considered to be great enough to noticeably affect production.

GOOD

II Temperature and precipitation are AB, BA, or BB. Soil is B, and terrain is B or better.

Whereas soil and terrain requirements are the same as a Ia potential, the periodicity of growth-inducing temperature or precipitation enables only one crop a year to be grown, thus reducing the natural potential yield per annum to one-half that of a Ia area.

FAIR

III Temperature and precipitation are AB, BA, or BB. Soil is B, and terrain is B or better.

Similar to type II environment except for a reduced soil factor. This latter is expected to reduce yield per land unit sufficiently below a Type II environment to warrant a lower potential rating.

MARGINAL

IVa *Land Marginal.* Three of the environmental factors, of which temperature and precipitation must be two, are B or better. The fourth factor is C.

Here temperature and precipitation may correspond to either a Type I, II, or III situation, the critical factor being either poor soil or rough topography. Either one of these factors, it is believed, will considerably reduce agricultural yield in spite of the favorable climatic situation.

The presence of poor soil or excessively rough terrain is given a slightly higher rating than where precipitation has a C rating, as in IVb below, because of the fact that here climatic conditions are reasonably reliable. Although soil may be quickly exhausted, forcing continuous *milpa* operations, or rough terrain may limit agriculture to such areas as low hill slopes and narrow valleys, nevertheless agricultural yield, although low, is reasonably reliable.

IVb *Dry Marginal.* Temperature, soil, and terrain are B or better and precipitation is C.

Here low precipitation may, under average conditions, be barely sufficient for dry farming. However, the unreliability of year-to-year rainfall typical of a steppe climate permits only intermittent agriculture, i.e. during the years of average or better-than-average annual precipitation.

IVc *Cool Marginal.* Temperature is C; precipitation, soil, and terrain B or better.

High latitude marginal agriculture caused by short growing season which, however, is ameliorated to some extent by longer days of growth-inducing sunshine.

SUB-MARGINAL

V Two or more of the environmental elements have a rating of C, or any single element is rated D.

Agriculture here can be carried out only with more than minimal technical interference with the natural environment.

With our four environmental elements classified, and their various combinations in nature rated as to their natural agricultural potential, we are now in a position to apply these ratings to various parts of the world. With this accomplished, we can then compare the cultural development in any one of these regions with its natural agricultural potential rating to see if there is any correlation between rating and cultural development. Should Meggers be correct in her conclusion that environment, through its effect upon agricultural production, controls the extent to which a culture can develop,[8] we should expect to find the areas of high cultural development rather consistently located in regions having a high natural agricultural potential. At the other extreme, regions having marginal, or sub-marginal, potential should exhibit a reasonably consistent pattern of correspondingly low cultural achievement. The accompanying list presents various regions of the world and gives their environmental element classification, their environmental complex, and the resultant potential rating.

8 Meggers, *op. cit.*, p. 815.

As may quickly be seen from this tabulation, there is very little, if any, correlation between potential ratings and cultural achievement. Certainly there is none in the Americas if we concern ourselves with prehistoric peoples. If anything is brought out in the above ratings it is that high cultural development has come about in areas considerably less than perfect for agriculture under natural conditions. The most startling in this respect are the region about Cuzco, the west coast

Area	*Temp.*	*Pp.*	*Soil*	*Terrain*	*Environment Complex*	*Natural Agricultural Potential Rating*
Prov. of Buenos Aires	B	A	A	A	BAAA	II
Region of Cuzco, Peru	B	B	B	D	BBBD	V
Coastal Peru	A	D	B	A	ADBA	V
Middle Amazon interfluves	A	A	C	A	AACA	IV
Middle Amazon flood plains	A	A	A	A	AAAA	I
Peten Guatemala	A	B	B	A	ABBA	III
Valley of Mexico	A	B	B	B	ABBB	III
Middle Gila Arizona	A	D	B	A	ADBA	V
Middle Tigris and Euphrates	A	C	A	A	ACAA	IV
N. W. Europe	B	A	B	A	BABA	III
S. E. China	B	A	C	B	BACB	IV

of Peru, and the Middle Gila valley of Arizona; all having a Type V or Sub-marginal rating. The first, with its single growing season, need for irrigation, and extremely rough terrain, is the scene of the great Inca development, while coastal Peru, with its virtual lack of any precipitation and only the flanks of streams as possible areas for naturally watered crops, is the scene of those great cultural developments of the Chimu, Nazca, etc. The Hohokam culture of the Middle Gila, although not comparable in development to those of the Peruvian coasts, nevertheless was of a much higher order than its Sub-marginal agricultural environment would presuppose.

The theoretical optimum of our potential scale includes the alluvial plains along the Amazon.[9] Rich in organic matter and minerals, these youthful soils are

9 Clarence F. Jones, *Brazil* (Focus, vol. 8, no. 6, pp. 1-6, 1958), p. 4.

constantly being renewed. With temperature and rainfall fairly constant throughout the year, these natural bands on either side of the river should, and did, allow permanent agriculture. However, the cultural development in these areas never was high, although population concentrations, at least in certain areas, reached considerable proportions: Omagua settlements, for example, once forming an almost continuous line of houses for 200 leagues along the Amazon.[10] Again, in the eastern Argentine province of Buenos Aires we have a Type II region which, at the time of the Conquest was inhabited by hunting and gathering peoples despite the presence of black prairie and deep chernozem soils and a temperate climate of the type found in our south-central United States.

Of particular interest, owing to erroneous presuppositions of long standing, is the potential rating which appears for the Peten region of northern Guatemala. Here, because of a tropical monsoon climate and heavy forest, it has long been assumed that the customarily accompanying leached soils also were present, and thus that an example existed of high cultural achievement in what was believed to be at least a Type IV tropical environment. Others have used this erroneous assumption to suggest that although the Maya penetrated the Peten as a more or less fully developed culture, this culture finally deteriorated as a result of poor soil.[11] Whether or not the Maya entered the Peten fully developed is not as much to the point as is the suggestion that poor soil drove them out of the area. From what can be gathered, the soils of the central Peten are of the rendzina type, a type considered to be one of the better soils of the tropics.[12] Because of the underlying limestone and the considerable moisture during the rainy season, lime is constantly being added to the soil through solution, and this, coupled with a naturally enforced fallowing period during the dry months of this monsoon area, makes this soil remarkably good for continuous cropping. In other words, the Maya did not enter, or develop in, an area of poor agricultural potential. It was not, to be sure, the very best environment, but neither was it marginal. In fact, on our basis of rating it had the same potential as the Valley of Mexico, although for different reasons.

Since soils play such an important role in presenting or withholding the prospect

10 Alfred Métraux, "Tribes of the Middle and Upper Amazon River" (in *Handbook of South American Indians,* Bulletin, Bureau of American Ethnology, no. 143, vol. 3, pp. 687-712, 1948), p. 689.

11 For the view that Maya culture was an autochthonous development see Milton Altschuler, *On the Environmental Limitations of Mayan Cultural Development* (Southwestern Journal of Anthropology, vol. 14, pp. 189-198, 1958).

12 C. L. Lundell, *The Vegetation of Peten* (Carnegie Institution of Washington, Publication 478, 1937), p. 8.

of extended cropping without artificial renewal, the positions of the interfluvial region of the middle Amazon and the region of southeast China, as noted in our listing, should be scrutinized. Although the former is in a tropical rainforest climatic situation and the other in a warm-temperate situation, both have the same marginal, Type IVb, potential rating owing to leached laterite soils. In the Amazon the lands require the constant use of slash and burn agricultural practice owing to quick soil exhaustion, yet in the Chinese area referred to continuous farming in the same fields has existed for centuries. If an environmental element such as poor soil does control cultural development, why has it not done so in China? The answer, of course, is cultural interference via the technique of manuring.

TECHNOLOGICAL DOMINANCE VERSUS ENVIRONMENTAL DOMINANCE

From the above-cited instances, and numerous others that might be mentioned, it would appear that it is not so much the natural environment as related to agriculture that controls the limits to which a culture might achieve, but rather the cultural environment. The presence or absence of agricultural techniques, and the cultural desire or the economic need to use such techniques to improve the natural environment, appear to loom large in the agricultural development of a given environmental situation. In the south Peruvian Andes, the home of the Incas, the steep topography which offered only narrow valley floors and small basins for simple agriculture was improved by the construction of numerous agricultural terraces. The lack of adequate moisture was improved by irrigation, which has the added effect of renewing soil fertility by bringing in new silt and soluble minerals to the irrigated land. By the addition of these culturally-derived techniques the potential ratings of limited parts of the area were raised from a Type V environment to that of a Type III, that is, one equivalent to that of the Peten Maya and the Valley of Mexico areas. Similarly, the areas adjacent to the river valleys of coastal Peru became agriculturally enriched zones through the application of irrigation. Here, as with the Hohokam of Arizona, the application of a single agricultural technique raised the environmental potential from a Type V rating to that of a Type Ib. In the Tigris and Euphrates valleys the construction of dams and channels and irrigation canals raised a naturally marginal environmental situation to a Type Ia, and with the increased agricultural yield, allowed the development of the great Babylonian Empire.

So many of the tropical regions of the world are associated with leached laterite or lateritic soils that to many anthropologists it has become virtually axiomatic that all hot tropical environments, whether they be of the rainforest,

monsoon, or savanna type, withhold from their occupants all but the most simple slash and burn agriculture, owing to the extreme rapidity of soil exhaustion under agriculture. It is true that the great bulk of soils in the hot tropics are of the leached type, but it is also true that some of the richest soils of the world are to be found in the flood plains and deltas of tropical rivers. Although environmentally poles apart, deserts and tropical forests have one characteristic in common where culture is concerned, i.e. continuous agriculture tends to follow the rivers. With the former, this is due to irrigation with the river as the water supply. In tropical forest areas the flood plains, many of them artificially drained, offer the richest soils because of continued renewal, and thus afford the possibility of sustained agriculture. The desert situation, not only because of its dramatic contrast with surrounding parched land, but also because it often has been the scene of high cultural achievement, has been obvious to all for many years. That the tropical forest situation is similar has not been as apparent, yet to prove it one need only mention such areas of intensive valley agriculture as the Ganges, Irrawaddy, Salween, and Mekong of southern Asia. Although sustained farming was practiced along the flood plains of various rivers of the Amazon basin, it here appears never to have developed into the intensive variety practiced in Asia. That it did not do so could be due to environmental factors affecting man's physical energy output, or it may be a purely cultural phenomenon.

That leached tropical soils have appeared to present an insurmountable barrier to cultural development seems readily apparent if one considers the Amazon alone. It is not just a matter of impoverished soils but their easy erodability as well. Heavy applications of green or animal manure and terracing in hilly country are absolute necessities. Coupled with these factors is the incessant invasion of second growth, and especially where firing is often used, the final incursion of grass which may present even greater problems to primitive agriculture than weeds or poor soil. However, to point to the absence of any real agricultural development in the leached soil areas of the Amazon as an example of environmental control is similar to pointing to the Imperial Valley before irrigation and proclaiming its natural dryness as evidence of environmental control. In the latter it became politically wise and economically feasible to culturally adjust the natural environment to the purposes of agriculture. In the Amazon, owing to numerous reasons, chiefly economic, no such cultural adjustment of the natural situation has taken place. However, with economic and political change the time could come when it might be feasible to develop this vast area. In other words, environment itself—in this case the element of leached soils—is not the primary cause for lack of development. The red soils of south China, as a substantiating example, have been worked for

centuries via the technique of intensive manuring. Here population pressure on the land appears to have forced the issue.

Let us again turn briefly to the Peten region of the Maya. Here we have shown that leached lateritic soils in fact do not exist because of the underlying limestone which has resulted in the formation of a relatively superior rendzina soil. Meggers, in her attempt to prove soil depletion as the cause of Maya abandonment of the area, tends to brush aside the results of the Carnegie Institute experimental *milpa* where soil depletion did not occur to any noticeable extent over a period of six years of continuous cropping.[13] Also, she has neglected a much more critical point. We will grant that the Carnegie experimental *milpa* at Chichen may not be wholly convincing because of its short duration. However, from the description given by Morley and Steggerda,[14] the experimental *milpa* was planted only to maize, and thus at least one highly important tropical agricultural technique for maintaining, if not improving, soil fertility was absent, that is, mixed cropping with a leguminous plant. This is a technique held to be highly important in sustained Asiatic tropical agriculture[15] and, owing to its widespread use by the American Indian, must have been developed early in this hemisphere. With the very good prospect that American agriculture originated in the tropics, it would seem foolish to assume that mixed cropping was purely a haphazard development on the part of the American Indian. It appears far more likely that with agriculture developing in a tropical environment where soils are most exposed to leaching and oxidation mixed cropping developed as the result of observation of the effects of mixed as against single cropping over a long continued period.

Regardless of how mixed cropping began, it is known to have a strongly favorable effect on the maintenance of soil fertility, and this, together with an already favorable rendzina soil, tends to support Morley's point that a field might well have been continuously cropped for seven years or more before it was abandoned.[16] Morley and Lundell's suggested factor of grass invasion in the agricultural lands of the Peten,[17] if true, presented the classic Maya with one of the strongest deterrents to primitive, plowless agriculture that is known. One need only remind the reader that the prairie lands of the central United States and east-central Argentina are

13 Meggers, *op. cit.*, p. 820.

14 S. G. Morley, *The Ancient Maya* (Stanford University, 1947), p. 152; Morris Steggerda, *Maya Indians of Yucatan* (Carnegie Institution of Washington, Publication 531, 1941), pp. 135-136.

15 Karl J. Pelzer, *Pioneer Settlement in the Asiatic Tropics* (American Geographical Society Special Publication no. 29, 1945), pp. 73-74.

16 Morley, *op. cit.*, p. 153.

17 Morley, *op. cit.*, p. 153; Lundell, *op. cit.*, p. 14.

areas of high natural agricultural potential (Type II in our scale), but until the plow was introduced no farming was undertaken owing to the inability to cope with the dense sod. In the Peruvian Andes the foot plow made it possible to invade the high grasslands, but until the time of that invention all agriculture was carried out below this level.

If the cause of Mayan cultural deterioration was not the soil but the gradual invasion of grass owing to the agricultural methods used, can we properly say that this cause was primarily environmental? Hardly. It was cultural inadequacy in the face of a new situation. In spite of the Mayan genius in art, architecture, astronomy, and, presumably, social organization, their inventiveness in terms of agricultural methods and techniques was woefully deficient. This is understandable, for up until the grass invasion the natural agricultural potential was sufficiently high to not warrant much technical development along this line. If we assume that at the height of the Classic Maya period agricultural fields were extended to their maximum feasible distance around the ceremonial centers, it is easy to understand that with the invasion of grass and a cultural inability to cope with it a slowly decreasing area of usable land would result. Eventually food production would be insufficient to feed the non-food-producers—a powerful stress under which any hierarchy must fall.

But if grass was the cause of this disaster, where is the evidence? The theory presents the spectacle of a great grass savanna at the close of the Classic Maya period. However, "once a grassland always a grassland" is not necessarily true, provided remnants of the original tree covering exist in and around the area. This was most certainly the case in the Maya country, and with the abandonment of agricultural interference, the forest began to invade the grasslands by natural seeding. Today, around Lake Peten Itza, there is still a large, environmentally unaccountable, area of grasslands which may very well be the remaining, as yet un-forested, grassland originally formed by the last independent Maya group, the Itza, conquered in 1697, and which remains a grassland owing to present agricultural practice.[18]

AGRICULTURAL POTENTIAL, A CULTURAL APPRAISAL

We have seen from the above that natural agricultural potential and degree of cultural development do not show the correlation necessary to validate Meggers' conclusion that environment controls the level to which an indigenous culture may attain.[19] On the contrary, we have seen that environmentally poor areas have been

18 Lundell, *op. cit.*, pp. 43-45.

19 Meggers, *op. cit.*, p. 815.

the scenes of high cultural achievement. Nevertheless, Meggers' proposed law, as written, would seem to hold true, i.e. "the level to which a culture can develop is dependent upon the agricultural potentiality of the environment it occupies."[20] That Meggers' conclusions about environmental control do not properly substantiate this law is due to the fact that she was thinking, apparently, in terms of "natural agricultural potential" instead of "agricultural potential," which is a different matter. The former refers to the natural environmental factors having to do with farming. However, "agricultural potential," *per se*, is a culture's appraisal of its local environment in terms of its own knowledge, agricultural techniques, and tools. Furthermore, since this cultural inventory changes with time, due to invention and diffusion, the agricultural potential of the region may change in so far as these new additions to the cultural inventory concern the agricultural use of the environment.

As for the environment's control of agricultural productivity, we may view it as being controlling in inverse ratio to the quality of the agricultural technology possessed by the occupying culture. For example, since we know that natural plant life is controlled by environment, we can confidently expect that the simpler the agricultural manifestation in terms of techniques and equipment the more closely will the situation approach natural plant growth conditions. Hence, the more will environment control crop production. However, the greater the understanding of nature, and the greater the agricultural technology and tool inventory of a culture, the less will be environment's control over agricultural yield.

Although a dynamic culture may improve its immediate agricultural environment by the development of suitable tools and techniques, this does not mean that it is capable of controlling all environmental situations. New environments present the need for new tools and/or techniques. Also, the greater the difference between the occupied environment and the new one, the greater will be the immediate environmental control, owing to the lack of understanding of the problems of the new locale. Meggers' reference to the environmental control factor in the Incas' inability to settle the *oriente*,[21] and the failing agricultural production of the Marajoara people in their new island home,[22] are illustrations of this point. These cultures were, so to speak, beginners in new environments, and thus at that simplest level of agriculture where, as we have noted, environment was free to control them as long as they were unable to change their methods or develop techniques more suitable to the new situation.

20 *Ibid.*

21 Meggers, *op. cit.*, p. 808.

22 *Idem*, pp. 808-809.

In the years since the last war it has become more and more evident that man, through his cultural processes, may control the more apparent factors of environment. However, in the broadest view, he is controlled, and will always be, by the laws within which nature, that is, environment, operates. The more man learns of his natural world, the more he will be able to manipulate it as long as he works within these laws of nature. It is no easy matter, for all of nature is linked together, including man, and the artificial manipulation of one element requires the manipulation of other closely related elements if nature's balance is to be maintained.

Museum of International Folk Art
Santa Fe, New Mexico

LINGUISTICS AS AN INSTRUMENT OF PREHISTORY

MORRIS SWADESH

THE PROBLEM

PREHISTORIC RESEARCH today benefits from an accumulation of techniques for discovering, correlating and interpreting data about the past. Each of them has its value and some are quite remarkable, but it is well to remember that no one technique can begin to compare in effectiveness with a balanced combination of all research means. Since only scattered small fragments of the past are preserved in the ground or reflected in the living cultural and physical forms of the present, it is necessary to bring to bear every possible line of evidence on each problem of prehistory. Specialists in each field of research should try to fight free from any tendency to limit their efforts to a favorite procedure or to close their minds to the clues derived from other forms of study and from other disciplines. However, open-minded receptiveness to the findings of other specialists is not sufficient, because, even when one wants to know what has been achieved in other fields, it is often practically impossible to find out, short of suspending one's own research for a period of years while one learns the language, the literature, and the complex customs of what is reported and what is left unreported among the practicants of each specialty. Hence, what we mostly need is for each of us to develop a full awareness of what his discipline has to contribute to prehistory, to concentrate on essential problems and to learn to translate out of the specialists' dialect into the general lingua franca of prehistory. In the present paper, we propose to discuss, we hope in clear language, the potential contribution of linguistics to prehistory, on the basis of the present state of development of this field, and the possibilities of further increasing its value.

There are three main ways in which linguistics can illuminate prehistory: (a) by establishing facts concerning the common origin and subsequent divergence of languages, implying the earlier unity and subsequent separations of peoples; (b) by discovering diffused features (of phonetics, structure, or vocabulary) among languages, which bear evidence of prehistoric culture contacts; and (c) by reconstructing the vocabulary of old stages of languages in order to bring out suggestions of the physical environment and content of prehistoric cultures. In each of these connections, matters of time and place are of prime importance both as intrinsic facts and as points of coördination between the linguistic and other lines of evidence. For all three aspects—time, place, and cultural fact—it is essen-

tial to have an evaluation of the reliability, so that users of the real or supposed evidence can distinguish the barely possible from the highly probable and in general to note the different degrees of probability needed to guide them in drawing conclusions from the totality of evidence.

LINGUISTIC MAPPING

If there is any part of linguistics of which archaeologists are aware, it is the matter of classification as represented on maps. These they evidently consult as a routine procedure, but their use of them is of necessity a hit-or-miss proposition. Sometimes the linguistic relationships prove to be enlightening with respect to archaeologic problems and sometimes not, so the scholar as a rule uses the data when they fit and disregards them when they do not. He assumes that there is an obscure culturological complication which explains the apparent contradiction or that the linguistic classification is wrong. However, in many cases the only real problem is that the map does not show time depth or shows it in inadequate fashion. Related languages which were a single tongue a thousand years ago may be shown in the same way as languages which have been separate for over five thousand. Maps which attempt to distinguish different degrees of linguistic divergence often do so with an uncertain and undefined scale of values or use a representational technique that is not readily grasped by the non-linguist. The problem of adequate determination of time depth has to be discussed apart; here we offer a few thoughts, not necessarily original, on how the facts may be shown.

If one wishes to make a map readily usable by the non-linguist prehistorian, it is necessary to base it upon a definite conception of time depth or at least of linguistic divergence, which is the linguist's means of inferring time depth. The scale may be based on estimated minimum centuries of separation or on such general degrees of divergence as close, distant, and remote, or on an equivalent differentiation of language groups into "families" (close relationship), "stocks" (distant), and "phyla" (remote).

One then selects a mapping technique to show such degrees. For example, one may represent each phylum with a distinctive color, differentiate stocks of each phylum by a distinctive hachure shown over the phylum color, and indicate the families by outline boundaries. Of course, in some cases the area of the family may be divided, but this will happen less often than in the case of phyla and stocks. As we shall see, setting up degrees of relation, if accurately done, is certain to run into the problem of overlap. Perhaps this can be shown on the map by the crossing of colors, hachures, and boundaries or by some other special handling of the ambiguous areas. Up to now maps have generally avoided the problem of overlap,

either arbitrarily to avoid difficulties in mapping or because linguists have not known how to evaluate it.

Instead of trying to show all the levels of classification on the map itself, it may be more convenient to indicate there only the narrower groupings, such as the individual languages and the families, or the families and stocks, and to complete the information in the map legend or in accompanying material. A useful device for this purpose is the relationship diagram, illustrated in the three examples which accompany this paper. Figure 1 shows points of contact among American Indian linguistic groups, according to the present state of progress of the author's research. The boxes enclose isolated individual languages, families, stocks, and micro-phyla whose point of closest agreement with the next closest entity is not less than 51 "minimum centuries" (abbreviated mc.); the maximum internal divergence within each group is shown by the numbers in the boxes. Figure 2 is a blow-up, with subdivisions, of a portion of the first chart, showing the Penan micro-phylum, with its subdivisions, Kalapuyan, Takelma, Yokutsan, Maidu, Wintuan, Klamath, Molale, and Sahaptin, and with its nearest external relatives, also sub-divided. Figure 3 shows a variant technique of diagramming, suitable for representing subtler dialect groupings.*

The use of the diagram in connection with a map can be facilitated by having the same identifying symbols—for example letters of the alphabet—on both. Insofar as possible, one should try to make top, bottom, right, and left of the diagram correspond with the location of the groups on the map. Repeating the map's colors and hachures on the diagram will also help.

During our efforts to define linguistic relations at all levels, we have sometimes been asked by archaeologists specializing in recent time-horizons as to the value of the more remote connections. Those who concern themselves with "early man," on the other hand, require the data of ancient relationships and particularly need a clear demarcation of these from the more recent. The general rule must be that

* Postscript re Figures 1 and 2: Continued research after this article went to press and before printing (September 1958 to January 1959) has resulted in several modifications of details of classification. Additional languages have been included, with the effect of reducing rather than increasing the number of groups, because in some cases they provided intermediate links in the chain of relationships. A few breaks in the continuity of the network have been closed up, for example, Zuñi-Keres has been found to be linguistically contiguous with Penutian and Mixean. With respect to Figure 2, Yokutsan has been found to be about equally close to Costanoan and Miwok, but slightly more with the latter than the former. A count of cognates between Yawelmani Yokuts and Clear Lake Miwok now justifies bringing their respective boxes into direct contact, which thus carries us back to a conception of Penutian agreeing with the original one of Kroeber and Dixon. The advanced version of the relationship diagrams will appear in some future publication.

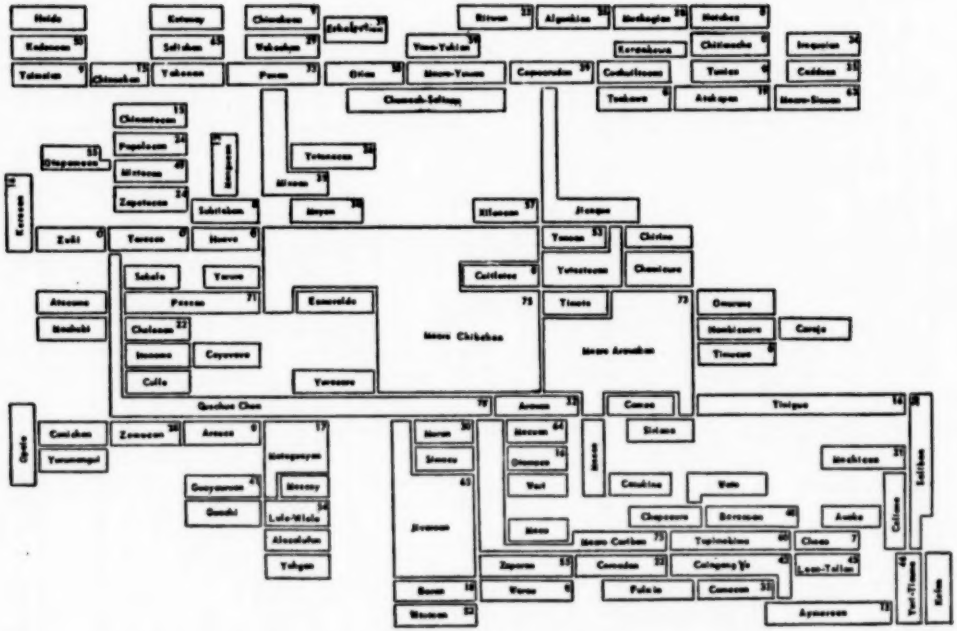

FIG. 1. Interrelationships of American Indian languages (provisional, September 1958). The numbers indicate internal divergences of each group in minimum centuries. Divergences between groups shown adjacently are from 51 to 61 minimum centuries.

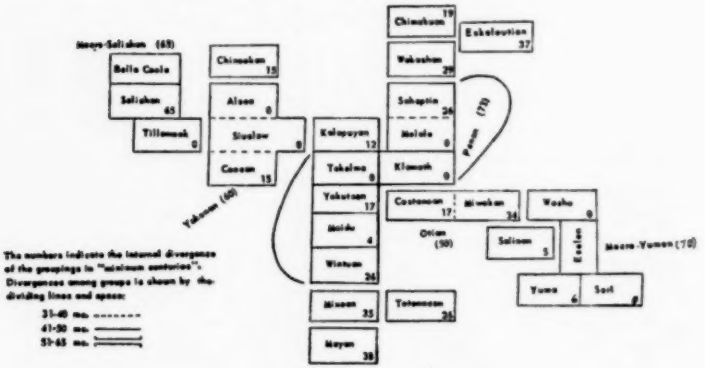

FIG. 2. Internal and external relationships of the Penan micro-phylum.

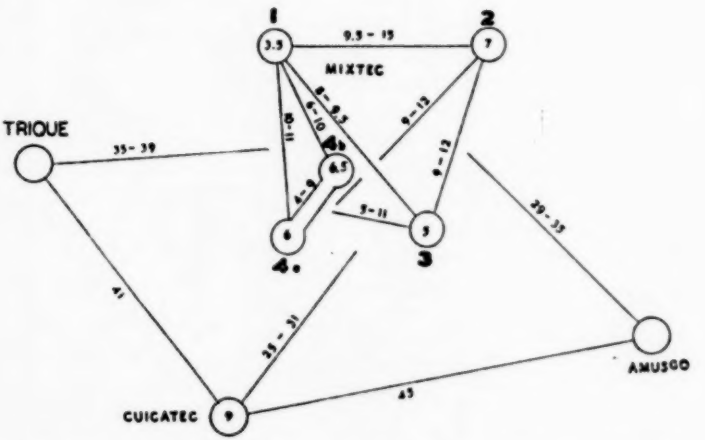

FIG. 3. Internal relationships of the Mixtecan stock (adapted from Evangelina Arana Osnaya, *Relaciones Internas del Tronco Mixteco*, dissertation, Escuela Nacional de Antropologia e Historia). Large block numbers identify the subdivisions of the Mixtec family. Smaller numbers within the circles give internal divergences in minimum centuries of each division; those on the connecting lines divergences among the divisions.

the prehistorian needs linguistic information for all periods back to the horizon of his particular problem plus one additional time stage, the latter being necessary to clarify the background. Since all time depths make up the total body of prehistory, comparative linguistics must study all periods, insofar as the present development of the science admits, and must at the same time bend a portion of its efforts toward devising means of penetrating to farther horizons.

PROVING COMMON ORIGIN

The principal requirement for good linguistic mapping is of course satisfactory linguistic classification. And logically basic to this is the matter of proving common origin. However, the fact is that in many cases the genetic connection of languages is so obvious that proving it is not a problem. Ordinarily, two languages which have separated say two thousand years ago still have so much common vocabulary, general structure, and specific morphologic elements that even a layman would know they are related. If one or both of them have undergone unusually great phonetic changes, it may require the insight of a practiced comparative linguist to recognize the cognate forms but for him they do not remain long hidden. If phonetic and morphologic change has been unusually small, the relation may be obvious for up to five or six thousand years. With greater effects of change and with greater time periods, the fact of relationship may still be fairly obvious if the comparativist has the good fortune to have available for study a large number of languages with interlocking relationship. Because of variable factors—affecting principally the amount of phonetic and structural change and the number and pattern of interlocking relationships—a classification like that of Powell and his associates is uneven in scale, failing to note separations of 3000 years in a few cases and recognizing others up to about 6000 years of time depth.

The most powerful technique for proving relationship in difficult cases would appear to be the systematic search for points of greatest concordance among the totality of languages. Where we cannot easily convince ourselves of the common origin of two languages or linguistic groups, we can look for other entities more closely related to each until we have built up a series of links involving unquestionable relationship, finally drawing our conclusion on the principle that languages having a common origin with the same language must have the same relationship with each other.

Where there are fairly great divergences in the chain of relationship, leaving a certain amount of doubt, it is helpful to set about building up a comparative dictionary. A collection of evident cognates linking various languages in the total set may add up to convincing proof of common origin. This was the method

used by Sapir in demonstrating the Hokan phylum to the satisfaction of most comparative linguists. However, since a few insist that the number of cognates is insufficient or that the presumed proto-language is insufficiently reconstructed phonologically, it is well to understand the quantitative requirements of proof.

It has been asserted that "phonetic laws" and not sheer quantity of resemblances demonstrate linguistic relationship. This is a half truth that requires clarification. It is of course true that comparative linguistics requires that supposed cognates (roots or affixes of common origin going back to the common period of related languages), in order to be accepted as real ones, must show correspondences of their component phonemes that are consistent with a unified and realistic theory of phonological development. Nevertheless, there has to be a scientific determination of the number of instances needed to prove the phonological theory in its totality and in its parts. Sapir's comparisons were strictly based on a conception of historic phonology and he reconstructed that of Hokan both in general and even with some minor details; the doubt cannot then be of the nature of the proof but of the amount. Since the first essential is to eliminate the chance factor (see below for the question of diffusional similarities), we need to calculate the number of agreements which might be contributed by sheer chance and still fit the phonological assumptions of the reconstruction. This can be done by taking into account the number of phonemes in each of the compared languages and their rules of combination into syllables and morphemes. We also need a criterion for how far the actual cases have to exceed the chance factor to guarantee within reason against the complicating factor of the fluctuation of chance. This problem has been solved at least in a preliminary way.[1] The general prehistorian can therefore be reasonably confident of distant genetic connections when he knows that they have met objective tests, as well as in the cases where the time-depth of the relationship is too limited to create the problem.

There has been much discussion of whether common vocabulary or common structure is the proper basis of proving common origin. This led Hymes[2] to devise a technique for calculating chance in the sheer order of elements in the word, whereby it is possible to prove common origin beyond reasonable doubt by structure alone in many cases. In the instances covered by the experiments, the matter could at the same time be proved by vocabulary. This confirms what lin-

1 For example, see M. Swadesh, *Problems of Long-range Comparison in Penutian* (Language, vol. 32, pp. 17-41).

2 Dell H. Hymes, *Positional Analysis of Categories: a Frame for Reconstruction* (Word, vol. 11, pp. 10-23).

guistic science would lead us to expect, that common origin is reflected for long periods both in vocabulary and structure. There may be some structures that lend themselves less well to mathematical proofs than others, just as the phonetic aspect is variable in this respect. Nevertheless, the existence of two separate techniques greatly increases the security and considerably extends the time depths and the number of specific cases that can be covered.

How far back can we trace linguistic connections with reasonable certainty? If in some areas the bulk of the comparative linguists confine themselves to problems belonging to the last five or six thousand years of time depth, this is only because of personal predilections and established areas of research. There can be no doubt that there are already successful probings of time-depths far greater than 5000 years, possibly up to 15,000, and that even more can be achieved by a systematic and careful application of already known techniques.

We have so far omitted specific mention of the complicating factor of borrowed elements, whether of lexicon or structure, which may sometimes give the superficial appearance of common origin between two languages. This problem can generally be met by distinguishing levels of stability in language material. Minor details of morphology and "cultural" vocabulary are more easily borrowed than broad structural features and "basic" vocabulary. Therefore, if the resemblances between two languages lie primarily in the area of what is readily diffused, the relationship must be due to borrowing; if it is primarily in the area of the most stable, it must be due to common origin. Often enough one finds both types of similarity, and sometimes along with them two different sets of phonological correspondences which permit us to distinguish what is due to common origin from the overlay of later borrowing. There is a need for much careful study of this problem, to improve our capacity for judging specific cases.

GENETIC CLASSIFICATION

The main service of comparative linguistics to prehistory is not simply asserting the common origin of languages but defining the degree of their divergence and relating it to two variables, time and separation. Time is a variable related to divergence in the sense that, under like circumstances, the longer the time the greater the divergence. Separation is variable in that it has different degrees of completeness from little more than zero to essentially complete; if parts of an original language community become completely separated the language in each of them will tend to diverge from that in the other more slowly than if they continue in occasional or frequent contact. Both time and degree of separation are of vital

concern to prehistory, and we must try to determine to what extent each is involved in the observed linguistic divergence.

Not all forms of linguistic differentiation are equally related to time and separation. It can be shown from specific instances that phonetic change sometimes moves with relative rapidity and sometimes is practically suspended. A condition favoring phonetic change is often the presence of a neighboring language or dialect with different sounds or with a given trend of sound change; consequently the presence or absence of the outside stimulus may determine the occurrence or non-occurrence of the change. Something similar is true of structural change, except that it is usually slower and less contagious. In vocabulary, we have to make a very sharp difference between the "cultural" and the "basic," the former being relatively sensitive to and the latter largely independent of outside contacts and cultural changes. Thus "basic" vocabulary taken in sum is the one part of language which most truly reflects the mere passage of time and is least affected by special factors in the history of each group, except that the specific changes may be influenced by the linguistic surroundings, particularly with reference to adjacent variant dialects of the same language. And because of this the lexicostatistic method[3] is a logical means of obtaining an index of the combined effect of time and separation. Based on the study of control cases and the determination of a norm of retention with respect to passage of time, the percent of cognates in the diagnostic lists of two compared languages is translated into a divergence measure called "minimum centuries," and representing the amount of time necessary for such a divergence to develop between two languages when they are not in a position of contact to influence each other. To the extent that the two compared languages have in fact been in contact, particularly in the earlier period while they are still mutually intelligible, the actual time may be from a little to very considerably longer than the minimum centuries of the divergence figure.

To some extent it is possible to distinguish the time and separation factors from a consideration of the pattern of divergence figures in a complex of related languages. When there are more than two languages and hence more than one measure of divergence in the group, the largest number of minimum centuries must be the closest to actual time. If two or a larger set of languages are relatively close to each other linguistically and if they all show essentially equal divergences with relation to other more distantly related languages, one may infer that they were an undif-

3 M. Swadesh, *Towards Greater Accuracy in Lexicostatistic Dating* (International Journal of American Linguistics, vol. 21, pp. 121-137, 1955); M. Swadesh, *Problems of Long-range Comparison in Penutian, op. cit.;* A. L. Kroeber, *Linguistic Time Depth Results So Far and Their Meaning* (International Journal of American Linguistics, vol. 21, pp. 91-104, 1955).

ferentiated dialect until after they lost contact with the others; their divergence with respect to the closest outside language must then represent a maximum measure of the time depth of their original unity. By using such hints in the pattern of divergences one can partially define actual time, but it is unlikely that a final answer can come directly from the linguistic facts alone. To complete the reconstruction it is well to make a coördinated use of the non-linguistic data.

The difficulty of separating the time and separation factors may appear to be a limitation on the usefulness of linguistic time-depth, but there is an even more valuable compensation in the possibility it opens for reconstructing the old dialect geography. The picture may be blurred due to the complexity of varying degrees of contact over space and time, but it is usually possible to translate a collection of divergence figures into a relational diagram which must bear some relation to the spatial collocation of the early dialects except that the order may be symmetrically reversed, giving a mirror image, and the orientation with respect to map directions cannot be known from the internal pattern of divergences alone. By broadening the comparison to include more distantly related languages, by considering the present location of all the languages, and by taking into account archaeological and other data, it may be possible to correct the orientation of the diagram and perhaps to fit it to a specific geographic area.

In the use of lexico-statistics as an instrument of prehistory, it should be borne in mind that it is a new method still needing much research and refinement.[4] The indices of divergence should be used with the necessary caution, and every effort should be made to check the results by other linguistic and non-linguistic evidence. In applying the method of divergences upwards of three or four thousand years, it is necessary to allow for a tendency to foreshorten long time periods. There also appears to be a "drift" factor, leading to a certain underestimation in shorter periods. Moreover, although diffusional influences do not ordinarily affect the basic vocabulary in sufficient degree to modify the total retention rate essentially, nevertheless special situations may occasionally modify this general rule; the distorting effect would be to exaggerate the divergence between the affected language and its congeners.

Before the advent of lexico-statistics, specific "isoglosses" were used to reconstruct old dialect geography. If two languages reflect the same archaic feature of phonetics, structure, or word usage, this may indicate that they were neighboring dialects in the old days. If two or more isoglosses give the same indications of dialect proximity, this constitutes confirmation of the reconstruction. Where a language shares isoglosses with two others, it may have been a geographically intervening

4 See Swadesh, *Towards Greater Accuracy in Lexicostatistic Dating*, *op. cit.*

dialect. The difficulty with isoglosses is simply that there may not be enough of them to fully reconstruct the old dialect map, and lexico-statistics has the advantage that it can be uniformly applied to each pair of languages in a group. Nevertheless, the use of the new method should not eliminate the old one but on the contrary the two should be used as counter-checks upon each other. In certain cases, the method of isoglosses will amplify the picture, reflecting later contacts after the initial period of dialectal relationship.

EVIDENCE OF DIFFUSION

It is often possible to prove, or at least to suggest with a certain probability, that common features in two languages are not due to common origin of the two languages but to diffusion from one to the other or to both from the same outside source. Such common features include sounds or the fact of undergoing the same sound change, relatively superficial details of structure, words of like form and meaning, similar associations of meanings in one morpheme (e.g. using the same word for *seed* and *eye*) or the same derivative formation to express a given meaning (e.g. *leg-head* for *knee*). If enough or sufficiently complex common elements are found to eliminate the possibility of chance, we may have evidence of cultural contact. The determination of when, where, and under what circumstances the contact took place may have to depend on other than linguistic evidence. There is sometimes the possibility of dating the contact linguistically. For example, borrowed words may show phonetic changes, whose relative date is known, and it follows that they came into the language before that time. Incomplete assimilation in phonetics or morphological treatment is usually a mark of recency.

The meaning of borrowed words is often an important clue as to the period, place, and nature of prehistoric culture contacts. If a language has many borrowed terms for local flora and fauna, it is an indication that it is new to the region and the source language has been longer in the region. Similar considerations apply to place names. Often one finds that the bulk of the place names are in the present-day language of the area, but that a certain number have no meaning in that tongue. These may be suspected of being borrowings taken from an earlier group of people. If possible one should try to find the source language. In certain regions, the bulk of the place names come not from the local languages but from a politically dominant group. In such cases, these names can help map the area of influence of that group.

The use of a borrowed culture term almost always implies the diffusion of the corresponding culture trait and is therefore a fairly direct clue to a fact of prehistory.

RECONSTRUCTION OF CULTURE

Once a group of languages has been found to be of common origin, their vocabularies can be examined for cognate elements that give evidence of the cultural content and physical environment of the old community. The number of helpful words will bear a relation to the recency of the differentiation, the cultural conservatism of the peoples, and the number of languages and the richness of the vocabularies available for comparison. One should not hesitate to apply the method no matter how divergent the languages may be, because even a few hints of early culture are worth having, but much more detail is normally recoverable in more limited time depths. Where the present populations have simple archaic cultures which are fairly similar one to another, the yield may be higher than in cases of advanced and much varied cultures. In making studies of this type, one should try to obtain material from as many related languages as possible and to get very complete vocabularies. It is well not to limit the languages to those going back to a common form precisely at the time horizon being studied, but to include also more distantly related tongues with a common period even farther back, since these may add to the clarity and completeness of the reconstruction. The parallel study of unrelated groups sharing the same cultural area may also be helpful by bringing out evidence of cultural borrowings and of interlocking cultural development.

The technique of cultural reconstruction from vocabulary is a complicated and delicate one,[5] requiring imagination and care in the linguistic aspect along with a good knowledge of contemporary and archaeological cultures of the areas concerned and a theoretical understanding of culture change. Collaboration between linguists and culturologists is to be recommended.

Two chief problems from the linguistic point of view are distinguishing real cognates from accidental similarities, and borrowings from old common vocabulary. Some cognates will have a probability so high that they cannot be reasonably doubted unless there is overwhelming cultural evidence against their antiquity. Others may be moderately reliable, moderately doubtful, or just barely possible. All should be assembled, because there may be other ways of verifying their archaic character but it is well to set down an estimate of the probability of each case, based on phonological considerations and upon the spread of occurrences among the related languages,[6] the latter not so much on the basis of present geog-

5 See, for example, O. Schrader, *Prehistoric Antiquities of the Aryan Peoples* (London, 1890).

6 See, for examples, Roberto Longacre and René Millon, *Análisis Cultural de los Vocabularios Mixteco y Macro Mixteco, Gloto-cronología y las Lenguas Oto-Mengues* (Cuadernos de Historia, Instituto de Historia, Universidad Nacional Autónoma de México, México, D. F., in press).

raphy as in terms of linguistic differentiation. As for interdialect borrowing, it is necessary to consider the possibility of this circumstance at every time level from the common period to the present. In many cases it is possible to recognize recent borrowings by the phonology, but this is less and less easy as one goes back to the common horizon. In the first period of mere dialects, the process of borrowing among local forms is hardly to be distinguished from the development of the language as a single undifferentiated whole, and unless the cognate element is also found in separate languages dating back to a still earlier common period, one cannot be sure it was already in use in the earliest phase of the period being reconstructed.

Cognates do not necessarily have the same meanings in the present languages. The instances of varying meanings may in some instances be the most enlightening of all, revealing culture traits now gone from all of the contemporary groups. The process of inferring original meanings from a variety of later ones, the semantic reconstruction, is thus just as important and just as demanding of caution as the phonological one.

Culture terms should be compared not only with words of the same category but also with non-cultural vocabulary. Where an artifact or cultivated product bears a name traceable to a common object of nature, this may be evidence of relative recency of the trait. The etymology may also reveal something of the original provenience, procedure of production, or customary uses. In addition to culture terms, words describing plants, animals, terrain features and phenomena of the weather (e.g. snow) may tell something of the ancestral home. A cognate place name may pin it down even more exactly.

POINT AND AREA

Eighty years after Johannes Schmidt[7] first pointed out the inadequacy of the old family-tree conception of language development, some comparative linguists still try to trace language groups back to single points of space and time from which previously undifferentiated parent languages sent out colonies to develop into series of daughter languages. This procedure fits only the exceptional case of a language carried from one point to a far off new location where it immediately loses contact with the old community; but normally the spread of peoples is gradual, with the old and new areas maintaining contact from generations to thousands of years. Innovations appearing in one part of a dialect community tend to spread all around, in some cases being eventually adopted everywhere and

7 Johannes Schmidt, *Die Verwandtschaftsverhältnisse der indogermanischen Sprachen* (Weimar, 1872).

in others reaching to various portions of the whole. Out of the complex series of processes arise dialects and separate languages, but the old continuity of dialects may be reflected thousands of years afterwards in the chain of interlocking relations among the derived languages.

The size of dialect and language areas and the reach of the uniformizing tendency evidently vary according to terrain and the level of economy and organization of the group. From a study of linguistic maps, it would seem that hunting-gathering groups, when not too much impeded by terrain, may long maintain a relatively uniform type of speech over a large area despite a low concentration of population and slow means of travel.

When dialect differentiation appears, as it must in time, it takes on the typical chain formation. Where the continuity is broken, it may be the result of the expansion of one group into adjacent territory, thus eliminating intermediate dialects.

In early stages of agriculture, with the appearance of fixed settlements, local differences develop in each nucleus of population, with gradations evidently maintained by the movement of people for trade and other purposes mainly among neighboring towns. Advanced commerce and political organization open the way to broader areas of linguistic unity, resulting either through the establishment of common trade languages, which eventually may become regional languages, or from colonization. Yet the kinds of development here suggested are merely broad generalizations, whose actual application has to be studied in each individual case.

The application of these general observations to comparative linguistics requires that we get away from the mechanical type of family tree diagramming and classification, develop techniques for handling chain relationships and adapt our conceptions of divergence, contact phenomena, and time-depth to the realistic possibilities of linguistic development. Instead of trying to force languages into discrete large groups, each subdivided into its set of smaller separate groups, we must look for evidence that will indicate what actually happened. The relational diagram, already mentioned as a technique for showing interlocking relationships of divergence, may be taken as a rough reconstruction of dialect geography, to be used together with reconstructed vocabulary and with non-linguistic evidence to help determine the location of the dialects and the subsequently developing languages in each period of prehistory.

NEEDS

Comparative linguistics, to make its proper contribution to prehistory, needs a great deal more knowledge of languages and of detailed genetic relationships than it now has, and much more than it can even hope to have at the present rate of

accumulation of data. That we need more people competent in this field and funds for field and subsequent research is all too obvious. However, even with increased personnel and facilities, a vital requirement is the development of more efficient methods of work.

Our field linguists must learn to collect good material faster—to obtain a working phonemic analysis in hours instead of months and to determine the main structural features of a language in days instead of years. In each aspect of the work—phonemics, structure, vocabulary—there should be an order of priority, putting principal emphasis on that part which is most essential for comparative purposes. Instead of trying to do a few languages with commendable thoroughness, we should find ways of making the data of all of them available with commendable speed. Survey and sampling techniques should be broadly used, and methods of exhaustive research applied at leisure, after a survey, not necessarily to all the languages surveyed but primarily to those which appear to be of strategic value for comparative research and to those which are in danger of being lost.

One of the great problems in efforts at comparing languages is that we may have very different coverages of material for the compared languages. We have the word for *horn* in one language and seek its equivalent in the next only to find that the recorder of that language did not obtain the word. In our linguistic surveys we should use standard vocabularies, well selected to obtain on the one hand basic words helpful in establishing genetic relationships and on the other hand culture terms useful in tracing old diffusional influences and in reconstructing ancient culture.

The use of standard vocabulary lists on printed (or mimeographed) forms reduces tremendously the work of the comparativist, since placing a set of lists side by side will reveal cognates at a glance which would take hours or days to locate in dictionaries or in ordinary field notebooks. However, since cognates often have different meanings, one needs in addition a guide to likely near synonyms and other related meanings based on knowledge of general semantics and the specific tendencies of particular groups of languages. These notions have led to the following technique of locating cognates, which the author has seen used with great saving of time and increased thoroughness. Word-lists for a set of languages are placed side by side, and all apparent cognates of the same meaning are put on a slip of paper; if no cognates are observed, separate slips are made for each word in each language. A carbon copy of the slips is retained in the original numerical order as a finder list; since the word lists are in topical order an alphabetical index by meanings is needed for locating the slips when needed. The originals are regrouped to bring together all those of related meaning, and cognates thus found are joined

by stapling the slips together. Sometimes different semantic groupings are tried until all the possibilities have been examined. Tentative reconstructions of phonetic form are written in for each set of cognates. The slips are refiled according to the phonetic form of the reconstructions, allowing the comparativist to study the correctness of the phonologic equations and also sometimes revealing additional cognates with unexpected meanings. Corrections in cognate identification and reconstruction are made whenever the accumulating information requires it. The file prepared for one linguistic group may be collated with another one to build up broader and broader comparisons. The technique we have described could be easily adapted to use with the simpler types of filing machinery, with the comparativist intervening at the critical points.

Prior to the operation of the foregoing technique, it is convenient to have a preliminary idea of what languages go together. This can be done by making tentative counts of possible cognates and grouping together all lists which show a given minimum percent of agreements.

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NEW EVIDENCE OF ANTIQUITY OF TEPEXPAN AND OTHER HUMAN REMAINS FROM THE VALLEY OF MEXICO

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IN JANUARY, 1957 Dr Luis Aveleyra Arroyo de Anda, Director of the Museo Nacional de Antropología in Mexico, and Dr Arturo Romano, Director of the Department of Prehistory of the Instituto Nacional de Antropología e Historia, Mexico, provided us with a series of bone samples of humans and extinct animals which have been recovered under conditions strongly suggestive of contemporaneity and high antiquity. In each case some obscurity and uncertainty over contemporaneity, and therefore antiquity, exists, and it was the hope that tests for nitrogen content and fluoride level of the bones would indicate some solution to the problem of whether the human remains were younger (presumably by reason of intrusion into older deposits) than the animals, or whether the human and animal remains were deposited at approximately the same time and are therefore coeval.[1]

As one control we determined the nitrogen and fluorine content of the femur from a human skeleton excavated by us at Tlatilco, a Preclassic site near Los Remedios on the outskirts of Mexico City.[2] This burial was accompanied by a number of stone and ceramic offerings which indicate that the grave refers to the final stage of the Middle Preclassic with an estimated dating of 700 to 500 BC. Wood charcoal collected from soil surrounding the burial has been radiocarbon dated[3] at 2525 ± 250 years old (568 BC).

The much discussed human skeleton from Tepexpan was represented in our tests by a rib fragment. No animal remains are associated directly with the Tepexpan skeleton,[4] but at a distance of about 2.6 km have been recovered two skeletons of mammoths, each of which was associated with flaked projectile points and other

1 A grant from the Wenner-Gren Foundation for Anthropological Research supported this investigation. The bone samples were secured incidental to a research trip financed by the National Geographic Society. A list of published papers dealing with the chronological significance of bone constituents is contained in an article by the present authors in this journal, Vol. 12, pp. 229-248, 1956.

2 On the Tlatilco site see Porter, 1953; Piña Chan, 1958.

3 Sample M-660 (reported in Science, vol. 128, p. 1120, 1958).

4 On the discovery, geology, and physical anthropology of the Tepexpan skeleton see De Terra, 1947; De Terra, Romero and Stewart, 1949; De Terra, 1957, pp. 160-171; Aveleyra, 1950, chap. 3.

FIG. 1. Location of find-sites. Approximate highest lake level after Lorenzo, 1956. (Scale approx. 1/100,000.)

chipped implements.[5] The two mammoth skeletons, which are clearly the remains of animals killed by man, lie in deposits identified as the Upper Becerra formation. Stratigraphic placement of the Tepexpan human skeleton is less certain since, although it lay in the Upper Becerra formation, some have claimed that the skeleton may be intrusive from overlying deposits of post-Becerra date.[6]

A third find, apparently from the Totolzingo formation which follows the Upper Becerra, consists of a mineralized skull encountered during well-digging operations near the village of San Vicente Chicoloapan at Km 29 on the highway between Mexico City and Texcoco.[7] At a distance of about 2 km from the find-site of the skull, in a sandpit, fossil remains of *Bison, Elephas,* and *Camelops* have been recovered from deposits which are assignable to the Becerra formation according to H. De Terra. The animal bone analyzed by us is from a *Camelops* skull.

The find of two human skeletons at Santa Maria Astahuacan by George O'Neill of Columbia University led to their recovery and study of the site by the Department of Prehistory of the Instituto Nacional de Antropología e Historia.[8] The skeletons were found in the side wall of a long-used open well or pond and the stratigraphic situation of the remains could not be, or at any rate have not yet been, satisfactorily determined. De Terra, who has studied the site recently, tells us that the soil layers in which the skeletons lay clearly lie on top of the El Risco sands which is a lacustrine beach deposit roughly of the same age as the Becerra formation.

A final analysis in Table 1 refers to the Xico human mandible found about 1890 in a sand quarry on the border of Lake Chalco near Astahuacan in the Valley of Mexico. The figures for percent of fluorine in the human mandible and the *Equus* radius found close by in the same stratum have been taken from an article published by Alfonso Herrera in 1893.[9] Aveleyra[10] discusses this earlier find, and concludes that we know too little about it to say definitely whether the *Equus* and human bone are of equal age. The close comparability of the fluorine content of the Xico horse and human with that of other human and animal bones which have been found more recently and for which we have some geological context makes it seem likely that the Xico mandible is in fact, as argued by Herrera, that of an ancient inhabitant of the Valley of Mexico. We would be in a better

5 Aveleyra and Maldonado-Koerdell, 1953; Aveleyra, 1956.

6 Black, 1949; Krieger, 1950; Arellano, 1951. For recent reviews of the late Pleistocene and Recent geology of the Valley of Mexico see Mooser, White, and Lorenzo, 1956; Krieger, 1957.

7 A report of this find has not been published. We have been provided information by A. Romano, P. Martinez del Rio, L. Aveleyra A. de Anda, and H. De Terra.

8 Romano, 1955.

9 Herrera, 1893.

10 Aveleyra, 1950, pp. 32-33.

position to support this judgment if the find site could be relocated and a geological study made, though this would be difficult and perhaps impossible after a lapse of nearly three-quarters of a century.

Any final evaluation of the significance of the chemical data as these relate to absolute age must await additional bone analyses and radiocarbon check-dates from the Valley of Mexico.[11] At this time we feel justified in making the following assessment.

TABLE 1

Nitrogen and fluorine quantities (shown as percent by weight) of human and animal bones from the Valley of Mexico

Site	*Type of bone*	*Nitrogen*	*Fluorine*	*Geological horizon*
Tlatilco	Human	0.72	0.046	Recent
Chicoloapan	Human	2.14	1.150	Totolzingo
Tepexpan	Human	0.06	1.540	Becerra
Ixtapan	Mammoth	0.02	1.822	Becerra
Chicoloapan	Camel	0.08	1.860	Becerra
Astahuacan	Human	0.08	1.988	post-Becerra (Totolzingo?)
Xico	Human	–	1.94	?
Xico	Horse	–	1.34	?

The Tlatilco results are roughly comparable to those already on record from the Sacramento Valley of California. In both places one can argue that the general environmental conditions and specific conditions of burial are similar, and it is of interest that the nitrogen and fluorine levels of human bones of about the same age from both areas are similar.[12] The Tlatilco site occupies a position near the foot of the hill of Los Remedios; all other sites considered here lie in the immediate lake basin areas and have been subject to greater influence of ground water.

The Chicoloapan figures for nitrogen we consider to be of doubtful reliability on account of the type of bone (a cancellous or spongy mandible fragment) available for test. It has been shown[12] that great variation of nitrogen level occurs within a single skeleton and that spongy bone tends to run higher in nitrogen than dense bone. The fluorine content does not appear to be subject to such wide varia-

11 Other possibly ancient remains are described in Aveleyra, 1950.

12 Cook and Heizer, 1952, p. 20.

tion according to the type of bone and we therefore view the high fluorine content of the Chicoloapan bone as a more reliable indication of its relative age.

The Tepexpan human, Ixtapan mammoth, and Chicoloapan camel bones all have been geologically associated with the Becerra formation and may therefore be considered as a group. The differences in nitrogen content of these three samples are considered as without significance—with such small quantities present differences of this magnitude are not important. The important point is that the nitrogen is essentially gone and in general terms this may be taken as clear indication of a long passage of time since the bones were buried. The rate of disappearance of nitrogen is unknown but the direction of the process (i.e. decrease through time) is clear.[13]

With reference to fluorine, we know that the process involves gradual uptake through time.[14] In this light Tepexpan man looks to be a bit younger than the Ixtapan mammoth and Chicoloapan camel. We are definitely of the opinion that Tepexpan man is ancient, and recent intrusion seems clearly to be ruled out. Whether Tepexpan man might be five hundred, or one thousand, or three thousand years younger than the Ixtapan mammoth we cannot say. The accuracy of the fluorine method is such that it is not possible to invest small percentage differences with precise time intervals. Since the Upper Becerra formation was deposited over a period of time believed to be four to five thousand years, interment of the Ixtapan mammoths and Tepexpan man may have taken place at different times and their remains still lie within the same geological stratum.

With reference to the Astahuacan skeleton, there exists a problem of correlating the post-Becerra deposits, in which De Terra says the bones lie, with low nitrogen and high fluorine levels which make it appear as ancient as, for example, the Ixtapan mammoths which lie in Upper Becerra deposits. Several alternatives may be proposed to account for the apparent discrepancy. Possibly the spring waters at Astahuacan are charged with fluorine, or the geological dating is incorrect. We are simply not in a position to say why the discrepancy exists and trust that further examination may lead to the solution of this problem. We are, however, of the opinion that the Astahuacan human remains are as old as the nitrogen and fluorine levels seem to indicate.

Finally, the Xico human mandible and extinct horse bones, whose fluorine contents were determined nearly three-quarters of a century ago in Mexico, may be taken as roughly equal in age to the Tepexpan-Chicoloapan-Ixtapan finds. Our interest in these finds is largely historical, since the chemical tests are not likely to be precisely comparable to our more recent ones. If, however, they are taken as

13 Cook and Heizer, 1947.

14 Oakley, 1951.

generally accurate, the remains would be assigned to a geological age equivalent to the Upper Becerra formation of the terminal Pleistocene.

Our results, while far from being conclusive, do seem to support the view of high antiquity for the Tepexpan, Chicoloapan, Astahuacan, and Xico human remains from the Valley of Mexico. Fluorine and nitrogen analyses will not provide absolute dates, but may be expected to indicate general magnitude of age if ancillary paleontological and geological data can be referred to.

More determinations of nitrogen and fluorine content of fossil bones from the Valley of Mexico are needed before the method can be said to yield unequivocal results. The fluorine levels seem relatively high for the age of the bones, and this indicates high fluorine content in the local ground water or soils. By combining chemical results with dates derived by the radiocarbon method we may look forward to further progress in this matter. The results presented here are an initial attempt and should be considered as tentative and subject to more precise evaluation in future.

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DAKOTA WARFARE

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IN 1935 when I studied a village of Santee Dakota in southern Minnesota, informants still remembered bitterly that their forebears had been driven from Lake Superior woodlands by the Ojibwa.[1] The struggle had begun 300 years before, and the prevailing competitive interest was still upon the generations of Ojibwa warfare, as upon sagas of a golden age.

The two peoples must have been in long juxtaposition, despite unrelated languages, for they shared a mass of cultural details. This was truest of the easterly Santee and the western Ojibwa. There was no time remembered by either when they were not neighbors, hunting rivals, and at war with each other. Apparently the Dakotas had held their own against the Ojibwa until the early or middle 17th century, for the Ojibwa feared and respected them, as evidenced by reports they carried to the French clergy and traders and by current tales. But with the coming of the French, the Dakota situation was seriously altered. For the Ojibwa entered into trade relations with the French, and secured firearms in exchange for pelts. With these arms, the Ojibwa dispossessed the Dakota of their Lake Superior territories.

Probably this new impetus drove the more western Teton and Yankton Dakota bands permanently onto the prairies and the more eastern bands of Santee onto the western fringe of Minnesota woodlands. In Robinson's history this view is "based upon oral traditions of the Tetons and Yanktons; maps of the 17th century and the overlapping land claims of the tribes."[2] Within the two centuries 1650 to 1850 the Santee had been pushed from the present northern Wisconsin and northern Minnesota to southern and western Minnesota. They might have been pushed still farther if not for the military intervention of the United States, as all 19th century accounts make reference to Ojibwa war parties that bore down upon the Santee in spite of the noted Fort Snelling at St Paul.

In 1862 occurred the disastrous Sioux Outbreak, resulting in the massacre of many Whites and the loss of much property, in the expulsion of the Santees from the state of Minnesota, and in the hanging of more than thirty Santee men judged guilty of murder. Whatever the detailed causes, it was an almost inevitable reaction to White pressure. The Santee were removed to reservations in the Dakota

1 The field study was conducted under a grant from the Anthropology Department, Columbia University Social Science Research Council. It was part of a program of studies I made of Canadian and Minnesota Ojibwa, Minnesota Santee, and Kansas Prairie Potawatomi.

2 Doane Robinson, *A History of the Dakota or Sioux Indians* (South Dakota Historical Collections, vol. 2, 1904), p. 20, n. 15.

states and Nebraska; in time they sold their allotments and the second generation was left impoverished. To relieve this, Bishop Whipple, Episcopalian missionary to the Ojibwa and eastern Sioux,[3] bought lands in the old Mystic Lake Santee territory along the Mississippi and invited the landless Santee converts to return. Some families did return, all as Episcopalians, and one of them at least took the surname of Whipple. They clustered at Prairie Island, a bald area in a valley about seventeen miles north of the present city of Red Wing, Minnesota. In 1935, some fifty persons were living there, again landless as the property had reverted to the Government for delinquent taxes; the people were very poor, almost wholly supported by county charity, and traditional life was practically gone. It was extremely difficult to secure information from any but a few of the aged, sulky members of the community. The present materials are based on work in 1935 with a few knowledgeable informants, supplemented by the historical but scattered publications and manuscripts about the Santee.[4]

The term Santee is applied to four Dakota groups or "council fires," which were distinguished from one another by slight removals in locality, dialect, tradition, and other cultural details. They included the People of the Mystic Lake, or mde'wa?kātōwā; the People of the Leaves or Woodlands, or wape?tō?wā? (Wahpeton); the People who Shoot Among the Leaves, or wax'be?ku'te or wahpekute; and People of the Swamps, or sisi'tō?wā? (Sisseton).[5] The term Santee from i'sa.?ti (or) i'sā.ti meaning "knife-bearers," was said to have been applied originally only to the Mystic Lake Sioux, but later it designated the group of four because they were recognized to be closely associated socially and culturally.[6] The present discussion bears principally upon traditional culture of the seven or ten Mystic Lake villages.[7]

3 Manuscript sketch of the life of Henry Benjamin Whipple, D.D., LL.D., by Grace Lee Nute (Manuscript Division, Museum, Minnesota State Historical Society).

4 Extracted from my comprehensive manuscript on "Santee Sociology."

5 Jas. O. Dorsey, *Siouan Sociology* (15th Annual Report, Bureau of American Ethnology, 1897), p. 215; S. R. Riggs, *Dakota Grammar, Texts and Ethnography* (Contributions to North American Ethnology, vol. 9, Washington, 1893), p. 180; Robinson, *op. cit.*, p. 23.

6 Riggs, *op. cit.*, pp. 159-160, n. 3; Dorsey, *op. cit.*, p. 215; Robinson, *op. cit.*, pp. 70-71; Rev. S. W. Pond, *The Dakotas or Sioux in Minnesota as They Were in 1834* (Collections, Minnesota Historical Society, vol. 12, St. Paul, 1908), p. 320; F. W. Hodge, ed., *Handbook of the American Indians North of Mexico* (Bulletin, Bureau of American Ethnology, no. 30, 2 vols., 1907-1910), pt. 1, p. 378; pt. 2, p. 460.

7 Pond, *op. cit.*, p. 321; C. C. Willson, *The Successive Chiefs Named Wabasha* (Collections, Minnesota Historical Society, vol. 12, pp. 507-512, 1908); A. W. Daniels, *Reminiscences of Little Crow* (Collections, Minnesota Historical Society, vol. 12, 1908), p. 513; Dorsey, *op. cit.*, pp. 215-216.

My field informant, Moses Wells, listed and named the ten villages of which he had heard his parents and grandparents speak.

The war pursuit fascinated most Santees, and to prowess by vision on the path were awarded higher honors than to skill in any other field; it sucked in or affected every major tribal value. All villagers joined in, except the berdache or wiŋkta, some heyoka clowns, those who chose suicide, women otherwise occupied, and the feeble.

The war pursuit played into major aspects of living: hunting, trapping, and berrying, manufacture of maple-sugar and rice, defense of the village, mourning dead kin or lover, brothers' rivalry over chieftainship, honors. The likelihood of war was at every turn of life. So was the liking for it, and village chief and elders were supposed to dissuade young men who desired it merely as a sport. But no penalty was attached to defiance; a peace-chief might feel no great sorrow if his own sons, through defiance, had been killed on the warpath. Indeed his sorrow could be transfigured to anger, leading to another, vengeful war feud. People who were not supposed to—were even forbidden to—followed war, like young and virgin women whose blood discharges were on occasion shamanistically-proven bad medicine; yet their coups were honored by male titles.

The reason for battle lay in the dogmatic assertion of all Dakotas that their warfare was defensive, chiefly to avenge trespass on deer- and buffalo-hunting territories and on trapping grounds, though none of these was owned privately, by individual, group, or tribe. But in the trespass, Santee had been killed, and this opened wide opportunities for feud-revenge. Consequently, Santee resorted to the warpath each spring and summer, when economic affairs were in abeyance, to avenge the death of sister, brother, husband or wife and children, or the capture of these. By extension (a principle of Santee thought reflected in the regularly extended categories of the Dakota kinship system), the deaths from war included deaths from age, illness, suicide, murder, and accident. That is, death was a loss and insult, and the mourner avenged himself or herself by killing or scalping an Ojibwa in war, "for they had killed our relatives"; and the soul of the dead enemy would serve to accompany the soul of the dead Santee in the after-life. At the victory dance, indeed, villagers treated an enemy scalp, and sometimes a captive, with the mournful tenderness shown the dead Santee with whom the scalp or captive was identified.

Thus, honors were given to those who warred or counted coup within the voiced ethical framework of revenge against trespass or other attack upon the tribe. Such people were "braves" even when female or young boys who had accidentally struck a skulking enemy by tossing a stone in play into the brush. People who recklessly killed, needlessly attacking an enemy visiting or being visited under truce, were rated foolhardy, and their claims to count coup not recognized.

Bravery, under the circumstances defined as honorable, was thus a criterion of mature responsible maleness. A good hunter was usually a proper warrior and could marry as soon as these abilities were demonstrated. For women, the "brave" analogue was childbirth though this was never explicitly titled. For a small number of women the temptation to male bravery was irresistible, especially if the family men seemed inadequate; and such women either went to war themselves, usually stealthily so as not to be deterred, or forced men to make up a party. That this was not common, despite the repeated stories, is suggested by 1821 population figures in Taliaferro's Papers[8] showing a preponderance of women and children over men in the villages, reflecting male casualties. The conventional female expression of revenge was suicide, and this was usually revenge upon a husband or father, celebrated not by titles but in legend. Conceptually, of course, this was an attack upon family and village, not a defense.

Sometimes young men committed suicide, and this was regarded as almost "cowardly." It was said that young men without war experience were forced to assume the dress of women at a village dance. A true man had to be a warrior, or else he was a berdache, that is, a transvestite who was exiled permanently from the village. There were indeed young men of war days who so dreaded war that they would steal out of the village in a band at the announcement that a party was being organized and recruits were desired. They were held in odium and were not supposed to marry; they did not return to the village until after the war party was gone. Unlike these, a male transvestite "had a dream to be like a woman, and he had to act like one or die." He was an exception but, like the female warrior, showed the difference in an acceptable way.

Consistent with "bravery" was the treatment of the aged and feeble men who crippled a camp's mobility and general welfare.[9] Young men, probably police appointed by the chief with the consent of the village council, presented the oldsters with bows and arrows and proclaimed them enemies. Ordering the old men to defend themselves, they fought and clubbed them to death. Apparently relatives raised no objections, or there were no relatives. The ends were served of honorable death in battle and of village relief. However it seems like an extreme procedure—of a village on the defensive yet careful to preserve ethical fictions.

8 Lawrence Taliaferro Papers, Journal 2 (Manuscript Division, Museum, Minnesota State Historical Society), p. 85. Taliaferro was an Indian Agent, and in Dec. 1835 he wrote H. H. Sibley: "The Sioux are the aggressors, not the Chippewa" (in Calendrical Summary of the Sibley Papers, Manuscript Division, Museum, Minnesota State Historical Society).

9 Pond, *op. cit.*, pp. 494-495; Mary Eastman, *Dahcotah, or Life and Legends of the Sioux around Ft. Snelling* (New York, 1849), pp. xix-xx.

To counterbalance war and violence were the institutions of village peace-chief and of beloved child. Two of my suicide tales concern beloveds of both sexes; no war chief was ever identified as a beloved. Peace-chiefs ran the frequent risk of being flouted, without any control over the situation, unless the village men, who were the council, supported them by appointing police. Tradition guided a chief when he was summoned to intervene for the peaceful settlement of a murder by compensation. His functions were advisory and admonitory, a reminder of the highest ideals. But when men's passions ran away with village peace, it was the shamans who were the final resort. Yet deviants seem to have been well provided for in this lusty culture that made niches for the men who were afraid and for the women who were not. The uniform atmosphere was that none could live in peace: men and women of the village were on the defensive and stood their ground; otherwise they ran away, were driven into exile, or committed suicide.

Open to all was the death-song, the last defiance. In the face of danger or death, from any source including scalping, suicide, murder, or hanging by United States soldiery, many sang or chanted a fragment which may have been of vision origin. Even girl suicides of legend are reported to have done so to keep up their courage. Riggs found "the noise . . . truly hideous"[10] made by the Santee ascending the scaffold to be hanged for their part in the 1862 Outbreak.

Many or most young men appear to have champed at the bit for the warpath. Properly they could not go unless accepted or invited by the leader of a party. But experienced men were reluctant to take novices because they did not observe discipline, were clumsy and frequently caused, or were "dreamed" to have caused, failure and even defeat. In true Dakota spirit, however, the novices forced themselves upon war parties, openly as servants or furtively, and sometimes won brave feathers. Their best chance was on large parties summoning manpower from all the villages; the four Santee bands often hunted and warred jointly, though sometimes villages raided one another.

Hunting and offensive war parties required experienced shamans as leaders; neither party was a function of the village organization though the personnel could include all the adult male population. In the early literature, Whites seem to have considered shamans as public officers.[11] Doubtless a war-leader was a village dignitary but not as a formal arm. Defensive war, against raids of Ojibwa or man-eating bears, showed the difference. Then village machinery stirred: the peace-chief named police to help organize defense, or the war-leader did. Under these condi-

10 S. R. Riggs, Papers (Manuscript Division, Museum, Minnesota State Historical Society).

11 Taliaferro Papers, p. 7, dated June 14, 1821, is an instance.

tions, village- and war-chiefs were sometimes bitter rivals, and Hughes[12] speaks of one such noted case that resulted in insurrection. The war-leader was always a visionary, usually Thunderbird, and offensive parties were his obedience to a vision injunction from which he could not be deterred in a single detail. A village-chief—even Shakopee, head-chief of the ten Mystic Lake villages—was flouted by war-chiefs and followers, especially if he tried to prevent unnecessary raids on the Ojibwa. On the other hand, when informed of an impending raid by Ojibwa, Shakopee would relay the news to the threatened villages and request tribal defense. Besides, every summer the head-chief made the round of the villages to advise the people how to live peaceably: "He always preached to them and the people always obeyed him."

Peaceable injunctions that supported village stability could not match the warpath with its opportunities to vent all kinds of grievances. Mourners went desperately on the path, to lose their despair in excitement and to make others suffer through death. Male mourners were forbidden, unlike women, to weep; but warriors did weep upon returning from an unvictorious raid, and they could weep in the victory dance over an enemy scalp which they equated with dead kin. Male mourners in fact often developed the dry-eyed stupor of their sadness into a pursuit of a war vision. They might lead or follow a party successfully after that, and occasionally this roused a lust for repeated raids until the warrior became a menace. In one case, the war lust brought reprimand from the village-chief, and resulted in murder and a division of the village. Besides, the young men seeking scalps or horses for personal glory only, sometimes violated peace ceremonies. There was no way of checking them, and even the war shaman might have to submit.[13]

War could begin under almost any provocation, from enemy raids while hunting and gathering wild rice to shamanistic quarrels at inter-village games and the fright of strangers visiting under a truce. Village shamans were often controlled and punished by police, as domestic enemies. The interchangeableness of the term "brave" by warriors and police shows the nuclear identity; often they were the same persons. Police punishment was called soldier-whipping in English, reflecting the Dakota idea. Police, scout, soldier, brave were described by the same term, shared with the Ojibwa, aĸitci'ta; brave alone was wa'di?da?ĸa. The nuclear brave-person had been trained for party or group discipline on war-party and hunt by the shamanistic leader, both alone as a vision-seeker, and in company with others; this included learning signals.

12 Thos. Hughes, *Causes and Results of the Inkpaduta Massacre* (Collections, Minnesota Historical Society, vol. 12, 1908).

13 Robinson, *op. cit.*, p. 77.

Police were always chosen from among seasoned warriors, strong from visions and from spilling blood in war and "mourning" over it (because of identifying war victim with deceased kin). It took such men to subdue shamans at village council and games. A village-chief usually left such matters to a war-leader and his soldiers. Once however Shakopee striped a shaman at the solicitation of other shamans because the offender was outdoing a pursuit of shamanistic revenge.

The sequence of war acts, like the war justification, was formal and clear. The leader's power was specialized from experience and constant communication with supernaturals; however, hunt leaders of necessity often functioned as war-leaders, and the techniques of hunting animals and men had much in common. Pond stressed the tribe's need of military alertness during the hunt.[14]

The war-leader called for followers, and at frequent stated intervals retired with his mature followers into a sweat lodge for vision communion. His was the major dream establishing all conditions; followers had lesser but vital dreams. Followers were supposed to obey the leader implicitly, and Eastman wrote that the war-leader had police to execute his orders.[15] The leader laid plans, organized the men, tested the young ones with fasting and drill-dancing,[16] distributed war honors, and shouldered all responsibilities. Actually in the heat of battle, the party broke up, individuals tending to dash ahead to make coups.

The party was organized into experience-groups, which my informant called age-groups. The leader always preceded at some distance, with his peace (or ceremonial?) pipe; he was the "oldest." Behind came the next "oldest," probably those next in experience. And so on, until the novices came at the tail. The party might be four or five days on the path and the leader always pointed his pipe towards their goal. Walking ahead, he stopped at the place he chose for camping that night; the others caught up with him. None could move from the spot except under the chief's orders, which took the form of the chief's picking up the pipe the next day after breakfast and walking ahead pointing it towards the enemy country. When the others saw him off at a distance, they followed, continuing thus until the last night of travel. Then the leader sat up and watched all night for the Ojibwa; his men sat behind awaiting a signal. At daybreak the leader loudly emitted the howl of a wolf or dog. After his last howl, the others rose and made for the enemy camp. They could attempt no greater number of tents or persons than stipulated in the dream. The great point was to scalp the

14 Pond, *op. cit.*, pp. 376-377.

15 Eastman, *op. cit.*, p. 153.

16 S. R. Riggs, *Forty Years with the Sioux* (Boston, 1887), p. 77—such drills as the no-flight dance.

enemy. Sometimes they exterminated whole families of women and children whose men were gone hunting. Then they stuck the babies' heads and hands up on poles. It was Santee mourners who practiced the more extreme of these mockeries.

The Santee ate the heart of a mud turtle to strengthen their own hearts and courage. They explained that the heart of this animal was so vigorous it continued to beat even after the rest of the body was cut to pieces. The leader sliced the heart into four quarters; young men were set around it and made to swallow it. If a young girl killed a turtle, she too was made to eat the heart. In 1935: "Nowadays no one eats the heart; that's why we are all cowards." But that summer Julia Owens gave one to her son, saying, "Now we will see who is the bravest. Eat some of this heart."

Their goals accomplished or frustrated, the war party returned home. Outside the village, the men stopped and stretched the scalps on hoops. The people saw them and went to meet them. They danced around the scalps with joy "because the Ojibwa did the same to their brothers, sisters, fathers," or other relatives. Mourning cries were always intermingled. They might dance thus for three days; then they removed the scalps from the hoops, stuffed them to "look natural," wrapped the scalps in good clothes (as though in completion of mourning for kin), and buried them on scaffolds.

There was great wailing at the return of both successful and unsuccessful war parties, because someone was killed, or because the memory of a recent death was stirred. Women alone wailed for relatives; it was they who screamed and celebrated. The men advanced with enemy scalps spread taut on little hoops and gave these to favored women who were relatives or sweethearts, and finally passed them around to all—men, women and children. The victory dance was always held outside the village, where the women met the party; there was eating, happiness, wailing, dancing, and much counting of coups.

Three coups were recognized, all about touching the enemy; and these were verified by the leader, who also sanctioned the use of eagle feathers and other insignia of brave deeds. Pond said the old men were inquisitive about how each wound was given, how each victim died, etc. At the dance, and later on when warriors gathered, a post or tree was prepared on which each recorded his exploits in pictographs and gave a verbal explanation.[17] Eastman wrote that the leader went into mourning for the enemy he had killed;[18] that children ornamented themselves and played with pieces of the dead enemy's body; that one Sioux woman had "cut to pieces the bodies of sundry murdered Chippewa children";[19]

17 Pond, *op. cit.*, pp. 449-450.

18 Eastman, *op. cit.*, pp. 226-227.

19 *Idem*, pp. xxix, 47.

that one 90-year old grandmother longed to accompany a war-party so she could mutilate Ojibwa bodies. Of three such applicants, one was rejected and another tried suicide and invoked the supernaturals against the Dakota war party.[20]

My informant said that the first of the three war counts went to the man who first touched a fallen enemy; for this he could wear an eagle feather that pointed skyward. The last two honors went to the men who next touched the enemy: the second wore an eagle feather turned earthward, the third wore an "Indian costume." These three men led the victory dance, followed by seasoned warriors of the village, singing traditional songs with variations celebrating events of the party, composed by the shamans. A brave's title, counts, and feathers were not transferable; some shaman gave him a song before or after the war-party, which also was not transferable. At this and later occasions of counting coup and singing songs of the party, the brave also enacted the situation of his victim—thus, lying down to represent the wounded Ojibwa—and all villagers who had killed enemies danced around him. At the end, the brave distributed gifts, and gave feathers to certain men, asking them thereby to drum for him while he sang. Then, at their discretion, these men drummed the songs of other warriors, and he to whom a song belonged had to rise and dance in place, after which he counted coup and gave gifts to "anyone." Braves with the greatest counts were beyond punishment by soldiers.

Besides these honors, a brave who had saved an unmarried woman from the enemy was entitled to her in marriage, being offered her by the parents or other guardian. In one story, the rescuer declined the offer, saying he was as yet unable to hunt well.

Female braves, though they made lone trips or joined parties under the rose, received the same honors for deeds as men. There was no feminine form of the wa'di$^{?}$da$^{?}$ĸa term. Like men they were given eagle feathers, counted coup and sang songs, danced with the men and like men, and acted arrogantly. Male braves tended to keep in the same parties yearly but not women. Though most women were fiercely interested in war, they followed the path only for special ends, such as avenging the death of a brother. One woman was honored as a brave because she struck and killed an enemy accidentally when tossing away the tibia of a deer; the Ojibwa had been skulking in the nearby timber, and next day they discovered his skull caved in from the blow. The woman received a feather when the next victorious war party returned, and joined in singing and dancing the "death singing" (wak'de.pi'doã.pi). Stories were told of fierce women warriors;

20 *Idem*, pp. 88-89.

one woman killed and scalped three Ojibwa on one trip, was honored with two feathers, "and was glad."

The Santee, like the Ojibwa, believed that "bravery" could be exaggerated to the point of "craziness," when the man, through vanity, disregarded serious hazards for love of war honors. People accorded him public honors which they qualified in gossip. One "crazy" brave was never married, supposedly because no one would entrust a woman to him. Women braves were never described as "crazy."

Captives were treated "kindly," as the enemy scalps were treated respectfully. The captive, identified with deceased Santee, was regarded "lovingly and sadly," given fine clothes and food. Sometimes a female captive was married. Generally the male captives were returned to the natal tribe; the Santee said they always went reluctantly after the attentions showered on them. The scalps were decorated, and even a comb was put in the locks of a female's scalp; after the victory dance, the scalp was buried honorifically.

The village-chief, as a peace officer, tried to treat captives well during their stay in the village; then he tried to expedite their departure to avoid the undisciplined anger of some braves. He always appointed a police escort to take the captives out of village bounds, and in informants' tales was generally successful but not always. Usually only one out of an enemy group was kept captive; then returned as a bringer of evil tidings to his people. The fine treatment he was accorded and the affection he was said to develop for his loving captors constitute an ironic twist that the Santee gave deliberately to a bitter stiuation. In one case, a party of ten men were taken captive and treated well.

And the Santee always felt sincerely that they were the offended party in any fray, no matter how much to others they appeared the aggressors. Their need to present themselves as the victimized party was very striking. No stories considered the Ojibwa position from this standpoint. But the Ojibwa were their preferred opponents, by implication as violent, resourceful, and nobly motivated as they. Their partnership in the game of war kept both tribes fit for the hunt and other life hazards. They visited and intermarried during truces and during captivities, keeping alive mutual understanding and an intense interest that gave special poignancy to their battles and sagas.

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THE COYOTEPEC *MOLDE* AND SOME ASSOCIATED PROBLEMS OF THE POTTER'S WHEEL*

GEORGE M. FOSTER

IF WE EXCLUDE the wheel, a Spanish introduction, and the simpler hand-building methods, two principal pottery techniques are known in Mexico:

1. With molds.
2. With a rotating support known variously as *molde* or *kabal*.

Both techniques have been well described and their areal distributions sketched.[1] The mold is found in a continuous area from at least the cities of Durango[2] and Colima on the west across central Mexico in a southeasterly direction to the Jalapa region of Veracruz, with a southerly border roughly corresponding with that of the states of Morelos-Puebla and Guerrero-Oaxaca. The molde is found in parts of the states of Oaxaca and Chiapas. The kabal, as is well known, characterizes Yucatán and Campeche, and it now appears that a Puebla instance must be noted (see below, p. 54). Molde/kabal techniques therefore form a loosely contiguous distribution area in southern and eastern Mexico, as contrasted to the mold distribution in central Mexico.

The molde and the kabal have been of special interest to pottery students because they suggest a possible step in the development of the potter's wheel. This hypothesis has been explored (and negatively answered) in three recent works,[3] but gaps in the data, particularly in the Zapotec village of San Bartolo Coyotepec (near Oaxaca City), have left several questions unanswered. In this paper I offer additional data, and discuss their implication. Molde and kabal are first briefly compared, as groundwork for this discussion.

The typical Maya kabal, as described by Thompson for Lerma (Campeche), is a wooden cylinder 10 to 16 cm in diameter and 6 to 10 cm high. The bottom of a kabal is rubbed with wax, tallow, or soap to reduce friction, and rests on a working board placed on the ground. The potters, both male and female, sit on a

* This is the first of two articles dealing with separate but related problems suggested by the remarkable characteristics of the Coyotepec *molde*. This paper is primarily descriptive, and serves as background for the second, essentially theoretical, which will appear in the following number of the Southwestern Journal of Anthropology.

1 Foster, 1955; Thompson, 1958.

2 Durango was not included in the distribution map in Foster, 1955. Pots which I purchased in the Durango market in 1958 clearly show a mushroom mold technique (perhaps combined with a wheel).

3 Foster, 1955; Shepard, 1956; Thompson, 1958.

low stool and turn the kabal with their toes or the soles of their feet, so that the position of the pot, which rests on the kabal, is adjusted constantly to the position most convenient for working it. Turning also is accomplished with the hands.[4]

Important variations in this "typical" kabal are found in Valladolid and Mama (both in Yucatán). In the former town the instrument is a wooden disc about 22 cm in diameter and 3-4 cm thick, flat on the upper surface and slightly convex on the lower. In the latter town the kabal is a pottery saucer averaging 22 cm in diameter and 5 cm high, in which is placed a wooden disc on which the vessel rests. Thompson points out that both of these kabals resemble some of the pottery and wooden moldes of Oaxaca and Chiapas.[5]

A kabal-like technique is also found in Acatlán (Puebla), a large and famous pottery-making town on the Pan-American Highway (Plate 1, no. 1).[6] Kabal pottery is formed either by hand-modelling an inverted cone of clay as it rests on the upper surface of the instrument, or by building with coils, or by a combination of both techniques.[7] Rotation speeds are very slow compared to the wheel. Shepard watched an experienced woman at Mama work at 10 revolutions per minute, and estimated 30 revolutions per minute as maximum speed for a man at Maxcanu.[8]

The simplest type of molde may be observed in Ocozocuautla and Berriosabal (Chiapas), where pots are formed on a flat circular board which in turn is revolved slowly on a larger working board.[9] This type of molde is very similar to the Valladolid kabal; its use differs in that it is turned by the hands working the pot rather than turning with the feet. In Santa María Atzompa (near Oaxaca City) a circular flat pottery molde, sometimes in turn rested on a larger circular wooden board which in turn is balanced on the bottom of an upturned pot, serves as base for non-coil ware.[10] Big coil-built jars are formed in large concave pottery moldes which revolve directly on the ground.[11] These moldes are suggestive of the Mama kabal. The most sophisticated molde technique is that of San Bartolo Coyotepec where a hand-modeled crudely-shaped vessel is placed in a convex pottery saucer, which in turn is balanced on a matched saucer turned upside down on the ground.[12] The vessel is finished by turning it by hand; since the area of contact of the paired moldes is minimal the potter must balance the pot while working. No molde, to

4 Thompson, 1958, p. 78.

5 *Idem*, p. 78.

6 I am indebted to Mr Antonin Brejník, pottery specialist with the UNESCO Fundamental Education Center (CREFAL) at Pátzcuaro, for this information, and for the photograph which clearly shows a male potter turning a *kabal* with his left foot.

7 Thompson, 1958, pp. 73-74.

8 Shepard, 1956, p. 62.

9 Foster, 1955, fig. 10a.

10 *Idem*, figs. 11c-d.

11 *Idem*, fig. 3.

12 *Idem*, fig. 8.

my knowledge, is turned with the feet: the potter's hands rotate the vessel, and normally they do not come into contact with the molde.

Until now the speed of rotation of Coyotepec moldes has been unknown, although at least one female potter—the famous Doña Rosa—was known to spin pots more rapidly than is customary for the kabal.

It is apparent that molde and kabal are similar in that both are mobile supports for pots, and both are turned so that the potter may sit or kneel in one place during the forming of the vessel, as contrasted to those techniques in which the vessel is stationary and the potter circles it. Molde and kabal differ in customary ways in which they are rotated.

Reëxamination of the question of a molde/kabal type of mobile base as a possible evolutionary step in the development of the potter's wheel is made necessary by the discovery that Coyotepec potters customarily rotate vessels they are working at speeds comparable to those of the true wheel. Such velocities with unpivoted turntables appear to be unique in America, and perhaps in the world. For convenience's sake the points to be discussed here are phrased as four interrelated questions, the first two dealing with lacunae in our Coyotepec data, the third with a relationship-distribution problem, and the last with the main theoretical problem:

1. Are the published accounts of Coyotepec work representative of the village, or do they represent a special case not valid for theoretical and comparative purposes?

2. What is the range of speed of rotation of Coyotepec moldes, and how does this compare both with the kabal and the wheel?

3. Are molde and kabal variants of one basic technique, which contrasts to the Mexican mold area to the north or, because of differences in turning methods, are they distinct techniques?

4. Is a molde or kabal type of instrument a possible evolutionary forerunner of the potter's wheel?

Question one has been asked by Foster, Shepard, and Thompson.[13] Its significance became apparent when we realized that the Coyotepec technique was unduplicated in any other part of Mexico, that it was perhaps the closest approach to a native American wheel, and that our principal evidence was drawn from the work of a single skilled potter. The basic study of Coyotepec pottery was, and remains, the 43-page booklet published by the Van de Veldes in 1939, a report quite acceptable by the standards of the time, and remarkably good in view of the fact that the authors were not anthropologists. Although the Van de Veldes speak

13 *Idem*, p. 31; Shepard, 1956, p. 61; Thompson, 1958, p. 143.

of the "potters" of Coyotepec their major observations appear to have been limited to Juventino Nieto and his wife Rosa, and only Rosa appears in photographs. They speak several times of the "spinning" of the molde and its vessel (but do not mention speed), and inferentially compare the molde to the wheel: "The spinning of the bottoms of the two vessels against each other is the only semblance of a wheel."[14]

I visited Coyotepec in 1940 and 1946, and on the latter occasion filmed the making of a pot. On both occasions the work of Doña Rosa alone was observed, and my published account is based on these observations.[15] Rosa's speed of spinning made me think of the wheel, but it did not occur to me to time revolutions. Revolutions per minute were not on the list of questions the student of native American Indian pottery customarily tried to answer. Skeaping's amateurish and brief account deals only with Juventino Nieto, Rosa, and their younger son Valente.[16] Sayles' fine photographs show a female potter, presumably the wife of one Crisoforo [Cristóforo?] Márques, but the text is too brief to indicate whether her work is comparable to that of Rosa.[17]

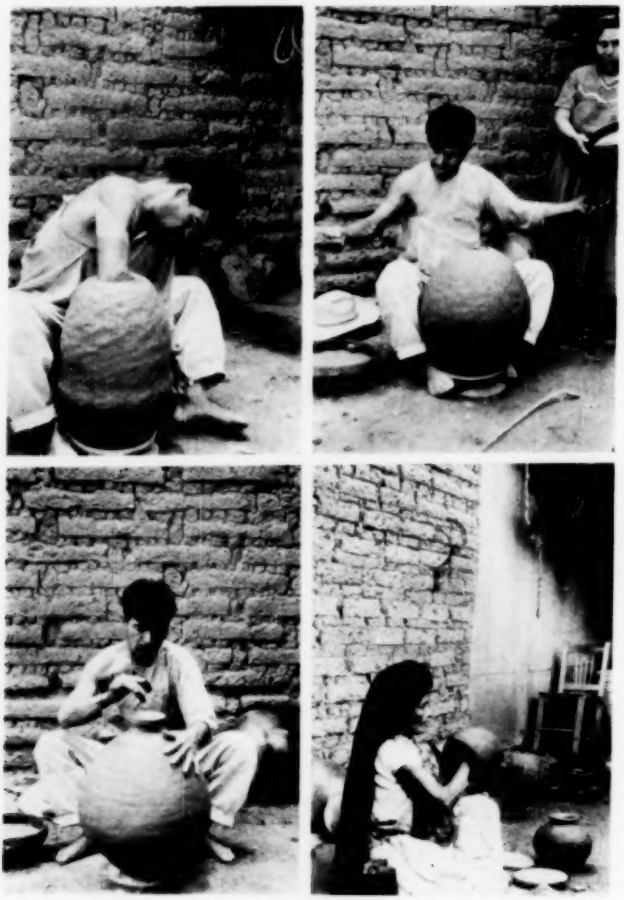
In short, our knowledge of pottery-making in a key village, and the theoretical conclusions we have attempted to draw from comparative studies, have been based on repeated observations of a single woman. These observations have been unsatisfactory for at least two reasons: the quality of field work, my own included, was not first class, and a sample of one is not a sample; no problem was recognized by field workers, beyond that implied in describing a process. A moral may be drawn from this: without definition of a problem simple descriptive field techniques, however satisfying they may seem to their practitioners, will usually be found wanting for comparative and theoretical purposes. When the problem of the possible relationship of a molde-type instrument to the wheel was recognized, the gaps in field data became apparent.

On July 25 and 26, 1958, my wife Mary and I revisited the village and spent a total of about eight hours watching four potters. We wished to learn whether Rosa was typical of other Coyotepec potters, if men worked in the same fashion as women and, above all else, how fast moldes were rotated. On this last point, as Shepard points out, the critical factor with respect to the forming of vessels in which rotation plays a part is "whether or not sufficient speed is attained for rotary and centrifugal forces to play a part in the shaping process."[18] In 1955 I wrote

14 Van de Velde and Van de Velde, 1939, p. 30.
15 Foster, 1955, pp. 22-23.
16 Skeaping, 1953.
17 Sayles, 1955.
18 Shepard, 1956, p. 60.

POTTERY-MAKING AT COYOTEPEC AND ACATLÁN

1. Potter using kabal technique in Acatlán, Puebla (photo courtesy of Antonin Brejnik). 2, 3. Elena Cruz working with moldes. 4. Maximino Mateo at work.



POTTERY-MAKING AT COYOTEPEC

1. Maximino Mateo expanding a *cántaro* by holding a *media-luna* pottery smoother against the interior wall of the slowly-turning vessel. 2. Maximino Mateo balancing molde and *cántaro* in kabal-like fashion with his feet. 3. Maximino Mateo spinning molde and *cántaro* at 60 revolutions per minute. 4. Doña Rosa pounding out the "rough unblocked felt hat" form, which is then placed on a molde for finishing.

"There is no question but that, at least for the smoothing of rims and necks, Rosa *spins* the molde and its pot with such velocity that motor patterns are much more closely related to those associated with true wheel throwing than with those associated with a mobile base."[19] In correspondence previous to my publication Dr Shepard had questioned whether sufficient velocity could be obtained on such an unstable base to achieve the ends I suggested. This doubt was natural in view of the fact that the implied velocity was several times greater than that ever reported for similar work. After seeing Rosa on my film Dr Shepard concluded that the force of rotation played a definite role in the formation of the neck of the pot, but that the motion of molde and pot was better compared to a gyrating top than to a wheel.[20]

On our 1958 visit to Coyotepec it was not necessary to ask for the house of Rosa. A sign on the main highway announced that the pottery of Doña Rosa was 500 meters to the left. We stopped a block farther on, however, and asked several women waiting for a bus if there were others whom we might watch making pottery. One, Doña Ramona Morena, said she lived just across the road and would be glad to show us. Ramona turned out to be a potter of modest talents, specializing with her husband and daughter in animal whistles and small *cántaros* (narrow-necked jars to hold mescal, a liquor distilled from maguey hearts). Her basic techniques proved to be the same as those of Rosa, as far as the initial forming of vessels is concerned, but her use of the molde was limited to simple turning, and in no way approached a spin.

In reply to our question as to who made large *ollas,* she named Don Maximino Mateo, who lived several blocks off the main highway, around the corner from Rosa. On our way there we passed a crudely-painted sign, *La Flor de Oaxaca* ("The Flower of Oaxaca"), announcing the pottery of Doña Elena Cruz who, it turned out, is visited by more tourists than anyone save Rosa. Elena is a highly-skilled potter who makes the beautiful flat bowls known as *cajetes* or *apazles, cántaros* of several sizes, and much bric-a-brac. Her techniques in forming a *cántaro* and in using moldes were essentially the same as those of Rosa. To determine revolutions per minute I fastened a daub of clay to the rim of the upper molde and counted its appearances while Mary recorded time on the sweep second hand of my watch. During the basic shaping of the vessel it was simply turned, but for forming and smoothing the neck Elena spun the *cántaro* approximately 10 times in 10 seconds, repeating this fast spin twice more. This gave an average of 60 rpms, and allowing for starting and stopping her highest speed would have been somewhat faster for five or six revolutions. When asked to spin the pot as rapidly as possible she

19 Foster, 1955, p. 31.

20 Shepard, 1956, p. 61.

reached a speed of nearly two revolutions per second, but this appeared to be faster than any normal working speed (Plate 1, nos. 2-3).

Maximino Mateo likewise turned out to be a master potter, specializing in large 25 liter mescal jars known as *cántaros de mescal de 5 medidas*. His wife, Doña Alberta Aragón, makes *apazles* and a niece hand-models small *cántaros* without using moldes. Both Maximino and Alberta referred to the moldes as *platos* ("plates") but they said that molde was also a perfectly common and acceptable term. Maximino's techniques, particularly with respect to posture, were sufficiently different from those of female potters to justify description in some detail.

Before beginning a large *cántaro*, Maximino removed his right arm from his shirt, allowing the sleeve to fall over his back. This was to avoid soiling the shirt. Then he seated himself on a low stool, in the fashion of kabal-users (but in contrast to women who seem always to kneel on the ground), and from a lump of clay roughly formed a vessel resembling an unblocked felt hat over his left fist. This seems to be a standard technique for all potters, both male and female, in Coyotepec. This vessel was then set in the uppermost of the usual two moldes, which rested between his outspread knees. While Maximino balanced the molde and its burden between the soles of his bare feet (also in kabal fashion) he made a clay roll about 45 cm long and 4 cm thick, hung it over his right forearm below the elbow, and took the other end between his thumb and forefinger. While turning the pot in a clockwise direction he pinched the roll on the inside of the top so that the pot grew approximately 5 cm in height in one revolution. What was left of the coil was then pinched thinner, and a second layer was added directly inside the first coil. Enough of the roll still remained for a third thin layer, which was worked down and inside the pot to thicken the wall of the hand-formed section. Two more similar rolls were added to the top, and worked down in the same fashion. There appeared to be no significant connection between the length of the roll and the circumference of the vessel; roll length is determined by the amount of clay that can conveniently be rolled out and balanced on the arm, and when affixed to the pot a roll has been extended to four or five times its original length. During this time the pot usually was turned in a clockwise direction, but occasionally it was reversed to smooth the surface or add a daub at a thin point on the interior wall. While the *cántaro* was being built up and laminated it remained cylindrical in shape.

The final height of the vessel was limited by the length of Maximino's right arm, working inside to the bottom of the pot (Plate 1, no. 4). This height was officially checked with two quick measures with the right hand with fingers extended, and one with fingers together (*"2 manos y 5 dedos"*). Then with his right

hand Maximino took a *media-luna,* a pottery smoother with a thumb indentation which gave it the form of a crescent moon. By pressing this against the inside lower wall of the pot, and slowly raising it, and by simultaneously turning the pot, the *cántaro* quickly took a spherical form (Plate 2, no. 1). The mechanics of this phase may be compared to those of a slow lathe: the tool, the *media-luna,* remained stationary except for the gradual rise to the mouth, pressing against and shaping the slowly revolving pot.

Up to this point the *cántaro* had been turned, sometimes quite rapidly, but never spun. Maximino frequently balanced it by cradling it between the soles of his bare feet as, for example, when he used both hands to make a coil, or when he picked up clay or rinsed his hands (Plate 2, no. 2). But at no time and in no way did he turn the molde with his feet, and in reply to a direct question he said no one did this in Coyotepec. Both he and Alberta agreed that his posture was usual for men, and that it was not characteristic of women, who kneel.

Since a large *cántaro* must dry for several hours before the neck is added, Maximino demonstrated this process on a second vessel made the preceding day. A small coil was slowly pinched around the 10 cm wide orifice at the top and roughly shaped with the fingers. The wide, horizontally-flared lip which characterizes these vessels was achieved by spinning the *cántaro* in a counter-clockwise direction. Maximino supported and held upright the vessel with his wet right hand, which also shaped the neck. Simultaneously he slapped the upper left body of the now-firm vessel with his left hand, pulling toward him (Plate 2, no. 3). The tempo can be described as pat-pat-pat, pause, pat-pat-pat, pause, and so on. Like Elena Cruz, he averaged 10 revolutions in 10 seconds on three successive operations to complete the neck, for an average of 60 rpms.

The momentum of the spin was astonishingly smooth, and it was this momentum that produced the centrifugal force that resulted in a lip as perfect as that of wheel-thrown pottery. Although power was applied discontinuously, the molde and its load spun evenly. It must be emphasized that it is the weight of the pot rather than that of the molde that makes possible this action. Small vessels cannot be finished in this fashion because they lack sufficient mass to spin smoothly.

A visit to Rosa brought our observations up to date. Here we were in for a shock, not so much for her work as for the change in the social setting. Four big tourist sedans were parked outside the house, which had grown from modest dimensions to large size. Inside we found a small, open air theater, admirably designed for photography. Rosa had set the tools of her trade against a plain wall carefully oriented for the morning sun, opposite which was a semicircle of thirty wooden chairs. Great quantities of black ware glistened on tables on each side and

to the rear, protected by a low roof. When we entered she was finishing a *cántaro*, to the accompaniment of the clicking shutters of a score of tourists' cameras. She looked much older, more wrinkled, and sharper in the face than when we had last seen her (Plate 2, no. 4); but she is now fifty-eight years of age, and no longer the young woman described by the Van de Veldes. When I asked permission to photograph she replied, "Of course, but don't forget my tip!" When the vessel was done she rushed to attend to tourist purchases, distracted for fear someone would slip out without buying. After 15 minutes another group entered and the pot-making demonstration was repeated. Her techniques were those described from earlier years. In finishing the neck of a *cántaro* she made approximately 15 revolutions in 10 seconds, or 90 rpms. It therefore appears that her spinning averages about fifty percent faster than that of two other good potters.

So many tourists visit Rosa that she and Juventino cannot produce sufficient ware to meet the demand, so other potters, including Maximino, sell to her. The artistic quality of her work, perhaps because of the speed with which it is ground out, has deteriorated since 1946, but we had the impression that this is also true of the work of the other Coyotepec potters. Rosa's standing with her fellow villagers also has deteriorated, and even those who sell to her accuse her of unfair tactics. Elena Cruz said that she (Elena) imitated Rosa and put a sign on the highway directing tourists to her house, but that Rosa tore it down. When I asked her why she did not put it back up she replied that Rosa would "strike" her if she did. Maximino said that Elena tried to start a coöperative, of which he was a member, but that because of Rosa's influence with the authorities they were unable to obtain a charter. The coöperative members then set up a sales stand on the highway on an informal basis, but Rosa, he said, persuaded the municipal officers to force its removal. We rather had the impression that Rosa's neighbors' opinion of her as a jealous, avaricious, bitter old woman was not incorrect. She is now famous, as much a part of the tourist rounds as Monte Alban and Mitla. But she does not smile, and she is feared and disliked by her fellow potters.

Returning to the questions asked on p. 55, the first two can now be answered with reasonable certainty. Previously published accounts of pottery-making in Coyotepec, although based primarily on the work of Rosa, are representative of the village, and hence are valid for comparative purposes. Rosa is probably the most skilled of the four potters we observed—she is far above Ramona—but her work falls within what must be a normal range for the village.

With respect to the third question, the relationship between molde and kabal, I continue to believe, as I speculated in 1955, that both techniques have evolved from a simple mobile base probably similar to that of Ocozocuautla and Berrio-

sabal.[21] The physical similarity of the two objects (and notably the Mama "saucer" kabal), the posture of men potters—particularly Maximino's kabal-like system of balancing with his feet—and the discovery of a true kabal in Acatlán, which puts Coyotepec within the distribution limits of this object, all suggest molde and kabal are variants of a common form and principle.

The most interesting conclusions, however, have to do with the possible relationship of the molde to a primitive wheel. Competent Coyotepec potters routinely spin moldes at speeds of 60 to 90 revolutions per minute, and the momentum of a good-sized pot is such that the neck (and the neck alone) is shaped by the motionless hand against which the spinning pot throws its weight. This speed is at least three times that of the fastest kabal speeds reported. But how does it compare with kick- (or "double-") wheel speeds? It is surprising how little attention has been paid to this seemingly obvious point. Pottery texts customarily ignore the matter, and among anthropologists only Childe offers a figure—"100 revolutions per minute at least."[22] Thompson appears to accept this figure,[23] and there is perhaps danger that this "standardized error" will become established in the literature.

Childe's figure is perhaps both right and wrong, depending on what he means by it. Wheel speeds frequently exceed 100 revolutions per minute, but throwing can be, and often is, done at much lower speeds. Actually, the speed of a potter's wheel cannot be spoken of as a constant, since it rarely rotates at the same velocity for more than a few seconds. In the standard American kick-wheel, with fly-wheel weighing 100 pounds or more, the potter accelerates the wheel to maximum velocity before centering the clay. As pressure is applied to the clay, velocity quickly diminishes, and it is necessary in a few seconds to remove the hands from the clay in order again to regain speed, and momentum. When the clay has been centered, and the potter first begins to dig into the top of the cone, and to form the vessel, speed again is increased, although often not to the velocity characteristic of centering. Less downward pressure is applied at this stage, so the wheel loses momentum less rapidly. Then, as the vessel is enlarged, either upward or outward, speed gradually decreases, since excessive momentum makes it difficult to control soft paste. The higher or wider a vessel becomes, the slower the wheel revolves, perhaps to the point where, to finish the rim, several seconds are necessary for a single revolution.[24]

21 Foster, 1955, p. 32.

22 Childe, 1954, p. 195.

23 Thompson, 1958, p. 141.

24 A kick-wheel revolves in the same direction for all operations; for a right-handed person this is counterclockwise, and for a left-handed person it may be clockwise.

On November 14, 1958, I conducted a series of tests at the California College of Arts and Crafts, in Oakland, timing the work of several potters with a stop watch. One excellent potter attained normal speeds up to 140 revolutions per minute before centering and before opening the cone of clay. Then, after opening the cone, and while doing the major forming, her speed ran close to 85 revolutions. For finishing rims this speed fell much farther. A second excellent potter achieved a top speed before centering clay for a medium-sized vessel of only 105 revolutions per minute. She opened cones at 90 revolutions, and drew up the sides at 60-80 revolutions. As an experiment she centered and threw a vessel of the same size at 60 revolutions. Technically there were no problems, but she said that it was "boring" to work at a reduced speed. Subsequently I timed her making coffee cups at normal working speeds for centering of 80-90 revolutions, and for throwing at 60-80 revolutions. Other potters less skillful than those described were clocked at 60-105 revolutions for centering and throwing.

Professor Edwin A. Cadogan, Head of the Department of Art, College of Marin, Kentfield, California, has given me the results of a series of tests on standard kick-wheels, utilizing an electric timer. His figures appear to be averages of longer stretches of seconds than mine, and do not reflect the extremes. Among "eight good potters" 80 revolutions per minute is about the top speed, and many good potters throw, he says, at much lower speeds. He found that he centers at about 80 revolutions, and does much throwing at around 60 revolutions.[25]

It should be noted that the top speed of wheels is not determined by the mechanics of throwing; high speed is the device necessary to obtain and maintain sufficient momentum for several seconds of work. A steady source of power makes possible much slower maximum speeds. The faster of two electric wheels I timed at California College rotates at 80 revolutions, and the slower at only 60. The second wheel has insufficient speed for large objects, but functions adequately for cups and other small vessels. Professor Cadogan told me that electric wheels in his shop run at 80-90 revolutions, top speed.

These figures indicate that Coyotepec potters work well within the speed ranges of modern kick-wheel potters for the finishing operation although, curiously, the points of highest speed are at opposite ends of the operation: the modern potter to begin, and the Coyotepec potter to terminate. A Coyotepec potter cannot throw a vessel, in the usual sense of the word, but he (or she) comes very close to throwing the neck. Moreover, it is clear that a pivot is not necessary for high speed, as is often thought. It looks to me, therefore, as if the molde, as a type, is a strong contender as a "missing link" in the evolution of the potter's wheel. How-

25 Edwin A. Cadogan, personal communication, 1958.

ever, so many factors are involved in demonstrating this point, and in drawing other related conclusions, that its full exploration must be left to a subsequent paper.[26]

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THE ROLE OF PROVERBS IN A NIGERIAN JUDICIAL SYSTEM[1]

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THE ANANG, in common with other African peoples, possess a rich folklore tradition, comprising most importantly tales, proverbs, riddles, and song verses. Proverbs are by far the most numerous and the most frequently employed of these forms of verbal art, and are used in all manner of situations—as a means of amusement, in educating the young, to sanction institutionalized behavior, as a method of gaining favor in court, in performing religious rituals and association ceremonies, and to give point and add color to ordinary conversation. Neighboring Ibo gave the Anang their name, the term denoting "ability to speak wittily yet meaningfully upon any occasion," and not a little of Anang eloquence, admired by Africans and Europeans alike, stems from their skillful use of maxims. This paper recounts a number of proverbs collected during court hearings, places them within their cultural and juridical contexts, and assesses their use as rhetorical devices affecting the course of justice in Anang tribunals.

Second largest of the six Ibibio-speaking tribes of southeastern Nigeria, the Anang possess no centralized political organization but are divided into twenty-eight sub-tribes, called *imɔn*, each of which is a group of villages ruled, to a limited degree, by a hereditary chief whose duties are mainly of a religious nature. The members of an *imɔn* share certain distinctive cultural traits and express a consciousness of unity; in addition, the meat of a particular animal is forbidden to them.

Politically preëminent is the community, or *obio*, rather than the sub-tribe, and a hereditary leader and a council of elders direct its affairs and perform important social and religious functions. The largest social grouping is the patrilineage, known as *ɛkpuk*, composed of both nuclear and extended families inhabiting a continuous tract of territory in the village. Each family lives in a compound surrounded by forest, bush, and land belonging to the head and farmed by his wives and children.

British rule over the Anang was established during the decade following the first contact of these two peoples in 1903. Native and Magistrates' Courts were gradually introduced which officially superseded the indigenous ones,[2] and con-

1 This paper is a lengthened revision of one delivered in 1957 before the Central States Anthropological Society, and is based upon research conducted in Nigeria during 1951-1952.

2 Most cases are tried in the Native Courts, the members of which are Anang elders, usually village and patrilineage heads, who have been assigned their position by the District Officer. They are sworn to uphold Native Law and Custom as delineated by the British—a body of statutes embracing both European and Anang concepts of morality—and their decisions are subject to review by the District Officer or appeal to the Magistrates' Courts.

tinual action has been taken by the colonial government to eradicate the latter, an effort that has met with only partial success. Two of the five types of traditional courts, called *esop,* have been abolished, but the other three still flourish in many communities. This paper treats only the indigenous judiciary, and the maxims under discussion were transcribed during proceedings conducted by these illegal bodies.

With the breakdown of *imɑn* political forms as the result of acculturation, the highest Anang court no longer exists. This was an *esop* composed of the chief of the sub-tribe and the heads of the communities within the village group. It convened two or three times a year to try the most heinous crimes—murder, failure to observe the food taboo, and the theft of objects used for religious sacrifice. Market courts as well no longer exist, for Anang markets are not numerous and thus can be policed easily by the British authorities. At one time, the leaders of *obio* contiguous to a market made up the tribunal and conducted hearings involving traders from their own and other *imɑn.* The crimes usually tried by this body were theft and usurping another's trading position.

The traditional courts still active are those serving the village, patrilineage, and family. Only a few *obio* tribunals continue to meet, and these sporadically, whereas patrilineage and family *esop* function in most communities as they did in the past. All try cases involving crimes not subsumed under Native Law and Custom, a state of affairs unofficially favored by some District Officers because they feel it allows the Anang to maintain a certain cultural equilibrium in a period of rapid change.

The head of the family and, if they reside in the compound, his adult brothers and sons form the family court, which convenes in the house of the compound leader at sunset on the day an offense is committed. The crimes most commonly tried are quarreling among wives, cruelty toward wives, disobedience of children, and petty stealing within the family group. The patrilineage *esop* meets weekly in a court building located in a small square central to the area inhabited by the kin-group. Presided over by the *ɛkpuk* head, this body consists of family leaders and sometimes old men who do not hold this position but who have high prestige. Most of the cases brought before the tribunal can be tried either here or in the village court, depending upon whether the principals are from the same or from different patrilineages. Chief among the offenses in this category are theft, assault with a deadly weapon, adultery, and causing an unmarried girl to become pregnant.

In the past most Anang adjudication took place in the community *esop.* It was composed of *ɛkpuk* heads presided over by the village leader, and met once each week in the compound of the latter or in the court house serving his kin-group.

Certain of the offenses which fell within its jurisdiction have just been mentioned: others of importance which could not be brought before other tribunals were: refusal to bear arms in inter-village warfare, striking parents or old people, performing sexual intercourse in the bush, practicing evil magic or witchcraft, and refusing to refund bride wealth following divorce. Today, in those communities where this *esop* continues to meet, the personnel of the court remains unchanged, and many of the crimes listed above still are tried, as well as some formerly falling within the province of the *imɑn* tribunal.

Village court procedures are much the same as those of other judicial bodies. Usually only older men and women attend the sessions, the former far outnumbering the latter, and children are barred except when they are principals or serve as witnesses. Women seldom speak out except when asked to furnish evidence, although a very old woman of high social standing might act as a character witness or advise the judges as to a proper verdict. Once the presiding member has introduced the case under consideration, the plaintiff stands before the justices, termed *ɛkpɛ ikpɛ*, and pays a small "utterance fee," used to purchase palm wine for the court members following adjournment. He states his grievance, without benefit of a lawyer, in a speech that may last as long as an hour, following which he surrenders the floor to the defendant who proceeds in like manner. When the *esop* itself initiates a case, the chief justice speaks as the plaintiff. The *ɛkpɛ ikpɛ* and those in the audience pay rapt attention to the litigants as they deliver their talks, and outbursts of applause mark the course of a well-presented accusation or defense. Listeners are especially appreciative of an original or little known proverb that captures their imagination and is cleverly introduced at a crucial moment.

The presiding member maintains strict control over the hearing, preventing interruptions when the principals are addressing the tribunal and allowing neither to dwell upon irrelevant matters. Both are then questioned at great length, first by one another, next by the *ɛkpɛ ikpɛ*, and finally by elders in the room, and at this time each may call forth material and character witnesses to buttress his position. When the chief judge decides that sufficient evidence has been introduced, he seeks the opinions of important old men as to the innocence or guilt of the defendant and retires with his colleagues to ponder a decision, returning with them to announce the verdict they have agreed upon.

The court may call a diviner to uncover hidden facts if it lacks sufficient evidence for a conclusive decision, or it may consult an oath swearer if it is apparent that one of the litigants or witnesses is falsifying his testimony. Often confronting a person with one of these dreaded specialists is enough to induce him to present

additional evidence or to alter his statement. If an oath is sworn, a trial must be postponed until the results of the oath can be ascertained. Periodically, *ɛkpɛ ikpɛ* are required to swear in public before a powerful oath administrator that they will neither accept bribes nor allow personal bias to influence their decisions.

The death penalty for a crime no longer can be exacted, but prior to the advent of the British those tried before the sub-tribal *esop* and found guilty were executed or, more commonly, sold into slavery. Lower tribunals were limited to levying fines and flogging convicted felons, this being the condition prevailing in the village court today. Fines are paid in cash or in domestic animals, both of which are shared by the justices. When a compensatory fine is ordered, there is usually a small accompanying punitive one that reverts to the members of the tribunal. As legal offenses are considered infractions of divine law, a guilty party, in addition to rendering a fine or being flogged, must perform a sacrifice before a particular shrine to placate the deity.

Now that we have examined in broadest outline the structural and procedural forms of the judiciary, let us discuss those factors which facilitate or hinder the administration of justice by the Anang courts. The strongest force acting to ensure equitable decisions is the belief of these people that their legal system is an instrument utilized by the deity, known as *ɑbɑssi*, to punish those who have failed to conform to his divine moral code. It is held that a miscarriage of justice in the courts can result in an aroused *ɑbɑssi* chastising not only the culprit, but also his relatives and even his fellow villagers. Thus, not only is the guilty one eventually forced to pay for his crime, but many innocent parties may suffer as well. Another supernatural sanction having great efficacy is the oath sworn by *ɛkpɛ ikpɛ*, for the oath spirit, at the behest of the deity, will quickly and ruthlessly attack any court member who engages in corrupt practices or who fails to act impartially during a hearing.

Democratic procedures certainly are evident in the conduct of a case. As we have noted, both plaintiff and defendant are given ample time to present their positions, and are forbidden to dwell upon extraneous issues that might unduly influence the justices. All aspects of the case are probed by the tribunal members as well as by interested elders through extensive questioning of the principals and witnesses. Pertinent evidence is examined with great care, the *ɛkpɛ ikpɛ* often traveling afar to view at first hand the site of a crime. Occasionally an authoritarian chief justice fails to call for opinions on a verdict from the audience and dominates his colleagues when a decision is considered, but most often he seeks the judgments of at least a dozen elders, as well as agreement among the members of the *esop*.

Most apparent of the shortcomings of the Anang judiciary is the willingness of some judges to accept bribes and the tendency of others to express bias, especially when kin are involved. The people readily admit that the Native Courts are corrupt,[3] but they insistently hold that *ɛkpɛ ikpɛ* of the indigenous tribunals have always been inviolate. It became evident that there is some discrepancy between the ideal and the real in this area after several months were spent in the field during which numerous hearings were attended and informants queried. Bribery and partiality are occasionally manifested, and probably were in the past despite the spiritual sanctions against them. The reasons for this are threefold: first, a very few have unorthodox religious beliefs and doubt the avenging propensities of *ɑbɑssi;* second, there are those whose pecuniary motives are so strong that they are willing to risk the consequences of immoral practices; and third, some possess charms procured at great expense from workers of evil magic which they believe can thwart supernatural retribution. Evidence also suggests that at times justices are prompted to declare the guilt of an individual when testimony fails to warrant it in order to share the fine, a condition that may have been more commonplace in the past when it was possible to sell convicted criminals into slavery for enormous profits. It is difficult to know how effective religious sanctions were half a century ago.

Anang justices of long standing were unconscious of and only slowly came to realize the influence of proverbs upon decision-making in courts. As the Anang take every opportunity to display their eloquence and constantly employ adages, it appears that such verbal expression is taken for granted and its impact thus not evaluated. Proverbs were collected which seemed to affect verdicts in particular trials; it must be admitted, however, that *ɛkpɛ ikpɛ* did not always agree with the researcher as to whether they had been unduly influenced by maxims in these trials, therefore only those hearings in which at least one justice admitted being swayed are reported on in the following section.

During a case in which a chronic thief was accused of robbery, the plaintiff aroused considerable antagonism toward the defendant early in the trial by employing the following proverb: "If a dog plucks palm fruits from a cluster, he does not fear a porcupine."[4] A cluster from the oil palm tree contains numerous sharp

3 The Anang attribute corruption in these *esop* to the fact that oath swearing in them was discontinued in 1947, as the result of missionary pressures, and salaries paid sitting members are low.

4 The linguistic data for this proverb and those to follow are found at the end of the paper, in the order of their citation.

needles that make handling it extremely hazardous, therefore a dog known to pick palm fruits certainly would be unafraid to touch a porcupine. The maxim implies that the accused is the logical suspect since he was a known thief and lived close to the person who was robbed, and many in the audience regarded the trial as a mere formality. His guilt came to appear doubtful, however, in the light of evidence produced during the proceedings, and just before the *ɛkpɛ ikpɛ* were to retire he presented an adage that was instrumental in gaining his acquittal: "A single partridge flying through the bush leaves no path." Partridges usually travel close to the ground in coveys and can be followed by the trail of bent and broken grass they leave behind. In using this proverb the accused likened himself to a single bird, without sympathizers to lend him support, and called upon the tribunal to disregard the sentiments of those in attendance and to overlook his past misdemeanors and judge the case as objectively as possible.

In another case the precept "Something happened to the smoke which caused it to enter the bush and become mist" was employed by a person accusing a former friend of assault with a machete during a heated argument. The plaintiff wanted the court to know that he disliked bringing charges against the defendant in light of their former close association, but was compelled to do so because of the severity of the attack. He compared himself to smoke which rightfully belongs in the compound but which has been forced out into the bush by the wind to become mist. Witnesses were introduced to support the accusation, and later the defendant claimed that they had been bribed by his opponent to testify in the latter's behalf. The accused attempted to show the justices that, although posing as a friend, the plaintiff was in reality a jealous enemy who had chosen this means to discredit him, ending his exposé with the adage "A leopard conceals his spots." Under ordinary circumstances a leopard resembles a large bush cat, but once angered it extends its limbs and arches its back revealing the distinguishing spots. The animal attempts to hide its identity by making itself as small as possible when stalking an intended victim. Thus the plaintiff was pictured as a dangerous foe hiding his true motives under the guise of friendship. The court was sufficiently impressed by this accusation in reverse to demand that the plaintiff and his supporting witnesses swear oaths as to the veracity of their claims, a demand they were afraid to comply with. When calling upon the specialist to administer the oaths, the presiding member appraised the case with the proverb "If an animal resembles a palm fruit cluster, how can it be butchered?" By this he meant that at this point in the hearing the evidence was so inconclusive that the *ɛkpɛ ikpɛ* would be unable to reach a verdict, just as an animal possessing the needles of a cluster would be difficult to

handle. The chief judge admonished the plaintiff and his supporters with the maxim "If you visit the home of the toads, stoop" when they expressed their unwillingness to take oaths, thereby forfeiting the case. This adage is similar in meaning to our "When in Rome, do as the Romans do" and is frequently used by a presiding member when pronouncing a decision, by which he emphasizes the necessity of conforming to the divine moral code, in this instance not bearing false witness.

In still another case a boy accused of impregnating an unmarried girl denied his guilt and accused a young man of having committed the act and boasting of it to other men in the *obio*. Not only did the girl contradict the defendant's plea, but the young man whom the latter had accused asserted his innocence. As the trial progressed, the case came to rest upon the girl's claim that the boy was guilty, especially after a number of witnesses admitted hearing the young man boast of his sexual exploits with her, exploits that she emphatically denied. The girl's relatives testified that her honesty was above reproach, and an oath might have been called for had not two witnesses spoken out using proverbs which influenced the judges to rule against the boy. The father of the boy, when called upon as a character witness, seemed reluctant to support his son, admitting that he often had punished the boy for sexual practices and telling lies. In light of this, he told the court, he considered his son capable of having committed the act for which he was accused and ended his testimony by using the precept, "The *nsɑsɑk* said she was ashamed of the small size of her offspring." Smallest of the birds in the region, the *nsɑsɑk is* thought by the Anang to be ashamed of its diminutive size. Naturally, they claim, the mother of the species is even more ashamed of its fledgling since it is far smaller than she. In employing this maxim, the father revealed his lack of faith in his son's claims and the great shame he felt at making the admission publicly. The proverb which finally persuaded the judges to declare the boy guilty was used by the girl's father when he was called upon as the last witness. He denounced the accused after telling the *ɛkpɛ ikpɛ* that her word was to be trusted absolutely, highlighting his speech with the proverb "The *ɛkɛnuk* tried to eat as much as the *ɔkɔnɔ* and his stomach burst." There are several types of rats in the area; one, the *ɛkɛnuk*, is only two or three inches in length, while another, the *ɔkɔnɔ*, is almost two feet long. The girl's parents likened the boy to the small variety and a married man to the large one; the youth is about to suffer for an act that only a married man is eligible to perform with his spouse.

Upon one occasion the misuse of a proverb by a defendant was instrumental in causing his conviction. A man accused of being an accessory in a theft became

incensed at the manner in which evidence was turning against him as the trial proceeded. After a particularly damning piece of evidence was introduced by a witness with the precept "When the fire burned the dog, it also burned the hunter holding the rope attached to the neck of the dog," the accused pleaded his innocence with the maxim "The snail is bleeding." Since a snail, lacking blood, cannot bleed when wounded, the defendant was asserting that he could not be punished for a crime which he had not committed. He continued by berating his opponent, the witnesses who had testified against him, and finally the members of the tribunal, saying that if the latter convicted him they would be guilty of misconduct. He employed the proverb "Overeating destroys the soul" in condemning the justices, implying that they would be going beyond what the evidence indicated and would thus invite the punishment of *ɑbɑssi* were they to rule against him. The *ɛkpɛ ikpɛ*, already disturbed by the inflammatory remarks directed at them, admitted that this precept consolidated their opinion as to his guilt. Once the verdict was announced, the accused in an emotional outburst admitted his involvement in the crime, claiming that he was not responsible for his conduct but was the unwilling victim of fate, and uttering the adage "The crayfish is bent because it is sick" to support his contention. In its natural condition a crayfish is elongated, but when trapped by fishermen and dried on a rack over the fire it curls up as a sick person might; the felon compared himself to the animal, compelled to do something against his wishes by a force over which he exerts little control.

These cases should suffice to illustrate the vital role played by proverbs in the dispensation of justice by Anang tribunals. In conclusion, it might be pointed out that adages play the same role in the Native Courts as they do in the indigenous ones, with like effect, and government officials appear as unaware of this obstruction to justice as the *ɛkpɛ ikpɛ* themselves. Before either judiciary can be considered equitable, Anang elders and colonial administrators must cope with this problem as well as with others, such as corruption, which are more obvious. It may be that the influence of proverbs is becoming less pervasive now that many justices in the tribe are aware of it. If this is so, it represents another example of the unpremeditated effect of anthropological research upon indigenous custom. There have been several instances of primitive peoples incorporating into their folklore tales related to them by anthropologists, and it is known that traditional theologies have been altered when inconsistencies in dogma were revealed by researchers. It may be that Anang judges will come to weigh more consciously the impact of maxims in arriving at their verdicts, for they pride themselves on the objectivity of their judiciary.

TEXTS OF PROVERBS[5]

If a dog plucks palm fruits from a cluster, he does not fear a porcupine.

ɛbuɑ	ɑmɑ	ɛbɛk	oyop	kɛ	ifɛrɛ	ibɑkɛ	ɛdiɔŋ
dog	who	plucks	palm fruit	from	cluster	fears not	porcupine

A single partridge flying through the bush leaves no path.

ikpɔŋ	ɑsɑsɑ	ɑmɑ	ɔfuɔrɔ	ɑfɑŋ	ɛsidɛ
lonely	partridge	who	after flying	path	closes

Something happened to the smoke which caused it to enter the bush and become mist.

ŋkpo ɑkɑnɑm	nsuŋ	ikɑŋ	ɛnyɛ	oduk	ikɔt	ɑkɑpɑ	otukubɛ
something happened	fly	fire	it	entered	bush	turned into	mist

A leopard conceals his spots.

ɛkpɛ	ɛdidip	nkɛmɛ
leopard	conceals	spots

If an animal resembles a palm fruit cluster, how can it be butchered?

unɑm	ɑmɑ	ɛtiɛ ntɛ	ifɛrɛ	oyop	ebɑk	diɛ
animal	who	resembles	cluster	palm fruit	butcher	how

If you visit the home of the toads, stoop.

ɛkpɛdɛ ɑkɑ	iduŋ	ikwɔt	esɔsɔrɔ	kɛnɛ	esɔsɔrɔ
if you go	village	toads	stoop	follow	stoop

The *nsɑsɑk* said she was ashamed of the small size of her offspring.

nsɑsɑk	ɛkɛbo	kɛbut	ntok	eyɛn	imum
nsɑsɑk	said	in shame	small	child	mute

The *ɛkɛnuk* tried to eat as much as the *ɔkɔnɔ* and his stomach burst.

ɛkɛnuk	ɛkɛbo	idiɑ	ntɛ	ɔkɔnɔ	ekpɑ	obomo
ɛkɛnuk	said	eat	as	*ɔkɔnɔ*	stomach	burst

5 The Anang consonants are as follows: b, d, f, h, k, kp (labio-velar plosive with no Indo-European equivalent), m, n, ŋ (as ng in sing), ny (as gn in agneau), p, r, s, t, w, and v. Its vowels are as follows: ɑ (as o in hot), *e* (as e in met), e (as a in hate), i (as ee in meet), ɔ (as au in author), o (as oe in hoe), and u (as u in rule). There is no a (as a in hat) nor ɪ (as i in hit). Long consonants and vowels are shown by doubling the letter (as in *ɑbɑssi*). Ạnang (Ibibio) is a tonal language, and I have employed eight tonal designations; however, they are unnecessary in a paper such as this.

When the fire burned the dog, it also burned the hunter holding the rope attached to the neck of the dog.

ikɑŋ	ɑmɑtɑ	ɛbuɑ	ɔfuɔrɔ	ɑtɑ	ɑkɑmɑ	uduk	ɛbuɑ
fire	burned	dog	fly	burn	holder	rope	dog

Overeating destroys the soul.

ukɑk	ɑdiɑ	ɛdɛ	uwot	ukpɔŋ
fllled	eat	is	killing	soul

The crayfish is bent because it is sick.

ikifɔnɔfɔn	obu	ɛnyɛ	ɔmukhɔ ɛkuŋ
ill	crayfish	it	bends

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THE PROPHET DANCE AND REACTIONS TO WHITE CONTACT[1]

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FOR SOME YEARS I have been concerned with the problems posed by Leslie Spier's work on the Prophet Dance[2] and have wanted to do further research on the topic. Wayne Suttles' recent scholarly article[3] has made me realize anew that I cannot accomplish this task without several years' residence on the West Coast with opportunities for field work and access to archives. Nevertheless, the issues raised by the work of both Spier and Suttles seem to me important enough to warrant a short paper, even though it cannot be buttressed by the sort of empirical data necessary to settle the questions raised here. If this paper leads to further analysis of materials now collected but as yet unpublished, or to the assembling of new materials—or to examination of other cult movements in the light of these comments—it will have served its purpose.

Spier's fundamental position in *The Prophet Dance* is a very cautious one, which leaves the reader with the possibility of drawing at least two quite different conclusions from his work.

> It has been generally assumed that the Ghost Dance, which in 1890 spread throughout the Plains from a source in western Nevada, was a wholly new development engendered by the need for a messiah at the moment and without historically connected antecedents. Such was the stand taken by Mooney. . . . For him other messianic or prophetic movements among the Indians were merely so many parallels which, with similar activities among Old World peoples, provided some notion of the psychic milieu from which messianic movements sprang. . . .
>
> It is the purpose of this section of this paper to show that the ultimate origin of the two Ghost Dance movements was not with the Paviotso but in the Northwest among the tribes of the interior Plateau area. It can be shown that among these people there was an old belief in the impending destruction and renewal of the world, when the dead would return, in conjunction with which there was a dance based on supposed imitation of the dances of the dead, and a conviction that intense preoccupation with the dance would hasten the happy day.[4]

1 The approach to cult movements outlined in this paper has been largely developed in the course of research on Navaho peyotism. This research has been supported at various times by the Social Science Research Council, the Wenner-Gren Foundation for Anthropological Research, research grants M-463, M-513, and M-2135 from the National Institute of Mental Health, Public Health Service, the Ford Foundation, Behavioral Sciences Division, and the University of Michigan. Part of the work was done at the Center for Advanced Study in the Behavioral Sciences at Stanford, California. I am indebted for valuable discussions or for reading and commenting on this manuscript, or both, to E. Kathleen Gough Aberle, S. N. Eisenstadt, Charles H. Hockett, David M. Schneider, and Albert C. Spaulding.

2 Spier, 1935.

3 Suttles, 1957.

4 Spier, 1935, p. 5.

Under this stimulus, from time to time men "died" and returned to confirm the doctrine, which would sometimes be held in high regard and sometimes be under the cloud of failure of prophecies to materialize.[5] Furthermore,

> it can be shown that the Prophet Dance was thoroughly at home in the cultures of the northwest, which cannot be maintained for the Ghost Dance in relation to Paviotso culture. It is also evident that basic elements of the complex were an integral part of Northwest Coast culture as well as that of the Plateau, which in turn argues for their antiquity.[6]

Finally, Spier says that a Christianized form of the Prophet Dance developed in the 1830's, and suggests the importance for this development of a group of Christianized Iroquois who settled among the Flathead around the 1820's.[7]

Thus far, Spier. Herskovits' interpretation of Spier's findings is more dramatic than Spier's phrasing.

> The supposition that the nativistic movements which since 1870 have appeared among the Indians of various parts of the United States constituted cults of despair, and represented reactions against social and political forces with which these natives could not cope, is sharply challenged [in Spier's *Prophet Dance*]. . . . What has been thought by many persons to have been a purely contra-acculturative phenomenon is shown actually to have arisen out of deep-seated aboriginal patterns which, even before the coming of the whites, had through intertribal acculturation come to be an integral part of the culture of at least the Indians of the Northwest, from whence the cult spread in its better-known revivalistic form first to California and then to the Plains.[8]

I quote Herskovits to indicate that according to at least one leading anthropologist, Spier had shown that neither the Ghost Dance nor the Prophet Dance were reactions to unfavorable contact with Western culture, but rather that they represented a development from indigenous roots in the Plateau which then spread widely through intertribal acculturation. Herskovits, however, seems to argue somewhat inconsistently. On the one hand Spier's work shows that the Ghost Dance was not a reaction to overwhelming effects of contact; on the other, the cult is called revivalistic. I shall return to this point in discussing dimensional heterogeneity below.

Spier's own more careful phrasing might be interpreted alternatively as meaning only that the Ghost Dance could not be accounted for *solely* on the basis of local conditions in the Basin and the Plains, but that we must also look for its external historical sources.

5 *Idem*, p. 5.
6 *Idem*, p. 13.
7 *Idem*, p. 30.
8 Herskovits, 1938, p. 76.

Suttles' paper represents a rather cautious support for Herskovits' position. He points out that many writers on nativistic movements see them "as responses to direct contact with a dominant alien society and consequent deprivation and frustration."[9] The Coast Salish form of the Prophet Dance was antecedent to any considerable amount of direct contact, but according to Suttles, indirect effects of contact may have created some social disruption before direct contact became important: the spread of epidemic diseases and the effect of the fur trade on social stratification are mentioned.[10] Suttles, however, is inclined to find the sources of cult movements among the Coast Salish in "a need for more bases for leadership" resulting from the poverty of Coast Salish society with respect to political institutions.

> This need may have led to occasional internal crises that permitted the rise of prophets who used religious concepts and ceremonies to exercise more than usual authority. This argument provides one more reason for concluding, as Spier did . . . that such phenomena may have been endemic in northwestern North America and not merely a response to the presence of Europeans. Herskovits' criticism of too much stress on White influence and on deprivation seems justified.[11]

Suttles has thus pointed out the existence and possible significance of mediate contact, but has laid his stress on native conditions as the source of cult movements like the Prophet Dance.

It is my thesis that currently available data do not afford a firm basis for explaining the Prophet Dance either as a response to "native" conditions or as a response to "contact" conditions, but—more important—that there are very strong theoretical reasons which make it impossible to *reject* the contact theory and which make further examination of the data in this connection imperative. Essentially my argument rests on the importance of considering mediate, as well as immediate communication, mediate as well as immediate trade, and deprivations other than those occurring when a group is overwhelmed, conquered, and totally transformed by contact.

COMMUNICATION AND THE PROPHET DANCE

If there is any one point that emerges from American diffusion studies, it is the length of the chains for the transmission of information under purely "native" conditions. The implications of this for the study of the Prophet Dance are that a group which has had no direct contact with Western culture may well "know" a great deal about it—although the information may be wildly distorted. The record-

9 Suttles, 1957, p. 392.
10 *Idem*, p. 392.
11 *Idem*, p. 393.

ing of the first date of direct contact for any group is always important, but it is not always a guide as a date for first knowledge. Fundamentally, we cannot know what the Northwest tribes knew about Western material culture, religion, or activities, prior to direct contact. To take a random example, the correctness of which is of no importance, if the Mandan were thoroughly involved with White traders, and if this contact were an element of major importance, for good or for bad, or both, in Mandan life, then it can be expected that the Mandan would transmit information about Whites and about contact to neighboring tribes. Since the information would be of obvious significance for their expectations, these tribes in turn would transmit it, and so on—so that tribes at vast distances from the original source of the information might "know" something about Whites and about Western culture.

Both Spier and Suttles show a clear awareness of the existence of long chains of communication about Whites and White culture. Spier's work shows us, indeed, the diverse and fantastic ways in which knowledge of Christianity was transmitted in some cases. The importance of the phenomenon, however, deserves to be underscored. First, it leaves us in the difficult position that we are unable to argue that a particular cultural event in a native group was *not* influenced by "knowledge" of Whites or White culture. We are not free to postulate contact where we know of none, but where we know that communication *is* possible, we are also not free to eliminate it as a possible cause of some of the events we observe. Concretely, we cannot be sure that cult movements are not responses to *knowledge* of Whites, even though they may not be responses to total cultural collapse resulting from direct contact with our society. This point will be further developed below.

TRADE AND THE PROPHET DANCE

By the same token, indirect effects of contact may make profound differences in native life long before direct contact occurs. The range of trade can be enormous —again a point well documented by American diffusion studies and by the archaeological record. The accounts of Lewis and Clark show us trade objects hundreds of miles from trading posts, and demonstrate, indeed, a set of overlapping trade networks reaching from the western outposts of American or Canadian culture to the West Coast.[12] Although neither Spier nor Suttles provides detailed information on trade, Suttles penetratingly observes, "It permitted hunters and trappers to accumulate wealth more rapidly than before and probably enabled them to rise socially at the expense of hereditary owners of fishing locations and other productive sites."[13] Thus trade may lead to changes in productive activities or in the

12 DeVoto, 1953, *passim*.

13 Suttles, 1957, p. 392.

balance of such activities, and to changes in the distribution of wealth and power, long prior to direct contact.

This means that mediate contact, involving what appear to be only a few sectors of the culture—e.g., trapping and the use of furs—may result in a wholesale transformation of the social order long before direct contact occurs. That direct trade occasioned at least some dislocations in the first quarter of the 19th Century seems to be indicated by the behavior of the Kutenai woman prophet mentioned by Spier, who announced that "the whites had cheated the Indians by trading whereas it had been intended by 'the great white chief' that presents alone should be made."[14]

DEPRIVATION AND THE PROPHET DANCE

It is perfectly possible that cult movements may occur in "native" groups in the absence of direct or mediate contact with Western culture. Unfortunately, in the case of non-literate groups it is almost impossible to gain any information about such movements. The first records of cults in "untouched" groups made by Western observers do not occur, in the cases known to me, under conditions where mediate communication and other types of contact effects can be eliminated from consideration. In the case of the Prophet Dance, none of our information can be genuinely regarded as completely uninfluenced by mediate contact with Western society. It might be argued, however, that even if it cannot be shown to be pre-contact (in my sense), it has nothing to do with deprivation. Spier himself seems to limit his view of deprivation to the sort of collapse of native culture before the overwhelming might of Western civilization so well represented in Mooney's *Ghost Dance*.[15] Suttles is aware of other types of deprivation that might have impinged on the Coast Salish, but is inclined to consider them of less importance than certain "native" political problems.

My own view is that we cannot eliminate deprivation from the scene of the Prophet Dance: that there are at least three types of deprivation, any one of which, or all three of which may have had important impact on early Coastal Salish and Plateau native groups—and on many others. In addition, there is a fourth basis for cult reactions which may or may not be a "deprivation." All four bases for cult reactions can be founded on mediate, as well as immediate contact. Finally, the three types of deprivation may be viewed by the native group as resulting from contact with Western culture, or may be interpreted as arising from other sources. Where the connection is seen by the native group, a cult response should contain some ideological reference to Whites; if none is seen, it should not. All three types of deprivation can occur long prior to the ultimate collapse of the native group.

14 Spier, 1935, p. 26, citing Ross. The episode occurred in 1811. 15 Mooney, 1896.

By a deprivation I refer to a negative discrepancy between expectation and actuality. Such discrepancies frequently arise out of changes in the contact conditions experienced by a native group in mediate or immediate contact with Western society, because of the extreme material potency of that society. Deprivation, however, is only one possible result of contact. At least temporarily, there may be "enhancement" of conditions. We cannot, in other words, assume deprivation when we know of contact. But, other things being equal, we cannot ignore the possibility of deprivation.

The first type of deprivation to which I wish to refer is that of a general worsening of the condition of the tribe, perceptible to its members, such that the subjective deprivation reaction takes the form, "We are worse off than we once were." Such deprivations may or may not be connected in the native mind with the impact of Western culture. They may well arise long prior to direct contact, as Suttles has pointed out. Epidemics of disease, crowding ultimately due to White pressure, and the like, may result in a worsening of conditions, and may well be seen by natives as stemming from Western cultural groups—though they may not be so perceived.

The second type of deprivation arises out of exposure to new wants which cannot be satisfied. The formula here is, "We are not as well off as 'they' are," the "they" referring to some other group used as a point of reference. The reasons for this situation may be attributed by the natives to Western culture, or may not; the criterion may be another tribe, and not Whites. Thus lack of arms, of trade goods, of horses, by comparison with another tribe, may well represent deprivation. It seems to me that this sort of deprivation is involved in "Cargo" type cult movements like those of Melanesia, where the deprivation is seen as coming from the Whites. Cargo cults, however, are not unique to Melanesia. Indeed, Spier's work provides an interesting example of what appears to be a North American cargo cult. Sir John Franklin reported in 1820 that one group of Chipewyans in about 1812 accepted the views of a prophet who said

> that there would soon be a complete change in the face of their country; that fertility and plenty would succeed to the present sterility; and that the present race of white inhabitants, unless they became subservient to the Indians, would be removed, and their place be filled by other traders, who would supply their wants in every possible manner. The poor deluded wretches, imagining they would hasten this happy change by destroying their present traders, of whose submission there was no prospect, threatened to extirpate them.[16]

The third type of deprivation arises out of differential shift of status in the tribe. Here it is not the condition of the tribe as a whole that is critical, but a situa-

16 Spier, 1935, p. 25.

tion where some members of the tribe are "worse off" in comparison with others than they once were—e.g., through uneven distribution of horses in a previously egalitarian group. This, too, can sometimes result in a cult reaction which will have selective appeal within the group. I have mentioned this situation of differential deprivation as a hypothetical result of contact, and Suttles has claimed its actual occurrence among the Coast Salish. In this context the odd linkage of a "marriage dance" with the Prophet complex becomes, theoretically, quite intelligible, although the present data evidently do not permit of a definitive solution.

Suttles points out that there was a "marriage dance" widely distributed on the Coast, which involved theoretically free choice of mates without parental intervention in the majority of cases.

> . . . It is the element that has stood out in the minds of most informants. . . . It may have been remembered by informants best because it is so clearly at odds with the usual Coast Salish marriage practices. The usual marriage was an arrangement between families of relatively equal status accompanied by exchanges of food, wealth, and privileges, and establishing a permanent economic relationship between them.[17]

If we take this conflict of patterns in conjunction with the alteration in patterns of stratification mentioned by Suttles, we can at least entertain the idea that a "free choice" marriage dance spread among the Coast Salish because some features of prior arranged marriages no longer made sense, and that this in turn resulted from changes in stratification patterns—based on the effects of the fur trade. I leave open the question of the interpretation of the Plateau marriage dance.

A fourth possible reaction to change in external conditions I have not labelled as a deprivation, although it might warrant the term. When a native group hears of a strange, materially and magically powerful new group which is having a variety of fortunate and unfortunate effects on other and more remote tribes, this knowledge can scarcely fail to produce an impact on the group. The problem for the group is that of assimilating and controlling ill-understood but potentially powerful forces which may affect one's own fate.[18]

All four of these conditions—three types of deprivation, plus ill-understood information about important impending events—*may* give rise to cult movements or expedite the acceptance of cults from other tribes. The conditions under which they *do* give rise to such movements are uncertain. I would assume that as a necessary, but not sufficient condition, there should be found an actual incapacity to

17 Suttles, 1957, p. 376.

18 Cf. the first reactions to Western material culture in a New Guinea tribe as recorded by Berndt, 1952.

cope with the deprivations by empirical means, with a consequent resort to religio-magical techniques: an assumption of considerable antiquity in anthropology.

In the case of the Prophet Dance and kindred movements, I do not have the information on the sequence of events resulting from contact to say which type of deprivation should be dominant in which tribe at what date. Nor does the information about the various cults tell us much about which kind of reaction is found where. Indeed, any one of the four problems may have existed, and cults oriented to any of these may have flourished. The historical accounts are often too vague, and informants' memories too leached by time to be sure. Nevertheless, Spier's and Suttles' data can be looked at from this point of view: there are features of the various movements which suggest the deprivations I have mentioned. There are what seem to be reflections of the deprivation of being worse off than before in a prophecy, which seem to be post-contact, of the destruction of the Indians by the Whites.[19] The deprivation of unsatisfied new wants, as I have said, is represented in the Chipewyan cult.[20] The marriage dance has already been mentioned in connection with the phenomenon of differential deprivation.[21] The fourth problem—anticipation of the Whites with their vast potentialities for good or ill, or both—seems to be reflected in a variety of prophecies about Whites, about material goods, about conquest, etc., all apparently prior to direct contact.[22] I cannot, in fact, be sure of the existence of deprivation for these groups, nor of the type of deprivation, nor of the content of the movement and its connection with the deprivation. But in the light of the content of these movements, we are surely in no position with the available evidence to *outlaw* deprivation resulting from changes in contact relationships as a basis for cult efflorescence, even though tribal life had not collapsed.

FORM AND FUNCTION AND THE PROPHET DANCE

Spier has suggested strongly that there is a continuity of form which reaches from the Plateau Prophet Dance to the Ghost Dances of 1870 and ultimately 1890. He seems to imply that this identity of form casts doubt on the importance of deprivation for the genesis of the 1870 and 1890 manifestations. It is quite possible, however, for there to be a considerable continuity of form—if Spier's chain of transmission is valid—with a very marked change in function: for the round dance, dreaming, world-end elements to be associated at one point with the problem of coping with confusing knowledge of the new culture, at another with inability to secure White goods, at still another with degradation of the group's condition, and finally with utter collapse and conquest of the group. Given perpetuation of the

19 Spier, 1935, p. 55.
20 *Idem*, p. 25.
21 Suttles, 1957, p. 376.
22 Spier, 1935, pp. 7-9, 16-17, 23-24, 57-60, 62-63.

form, this reinterpretation might occur within the same group, or in transmission from group to group. But our present information makes it highly plausible to see one common element uniting all these diverse functions: it still seems perfectly reasonable to see the Prophet Dance-Ghost Dance continuum as always attempting in one form or another to cope with the problems created by ever-changing, but ever disturbing direct and indirect contacts with Western culture.

DIMENSIONAL HETEROGENEITY AND THEORIES ABOUT THE PROPHET DANCE

Fundamentally, Herskovits' interpretation of Spier, and perhaps Spier's own approach, contain one serious error. Suttles has avoided the error. The problem seems to be phrased in the following form by Herskovits: If we can show that the 1870 and 1890 Ghost Dances have historical antecedents outside the Basin area, we can thereby prove that the Ghost Dance did not arise out of the deprivations of the contact situation. In other words, either a thing is borrowed or it is a reponse to deprivation. In this phrasing the error becomes plain: it is that of implying a dimensionally heterogeneous equation. Propositions which make this error in crude form are easily detected; others, far less so. Thus, if I were to say that men can be classified as "fat" or "bald," and were to continue by saying that since Smith is bald, he must not be fat, my error would be immediately observed. But if I ask whether culture change takes place through evolution or through diffusion, the two-dimensional character of my question is by no means easily apparent—or seems not to have been so for a very large number of anthropologists.[23]

In brief, we have at least two dichotomous dimensions concealed in the apparent "either . . . or" proposition stated above. The content of a cult may be native or borrowed; the cult may be rooted in deprivation resulting from contact with Whites, or it may not. The second dimension, however, can clearly be resolved into smaller components: rooted in deprivation resulting from contact with Whites, or in deprivation resulting from other sources, or not rooted in deprivation. In this paper I have expanded the deprivation dimension by providing a classification of three types of deprivation. When this classificatory system is applied in the context of the present discussion, my argument is that the Prophet Dance in its earliest form *may* be local in form *and* be based on deprivation, and indeed on any of the deprivations I have mentioned. The Ghost Dance, on the other hand, may well be borrowed, as Spier says, *and* be based on my first type of deprivation. Classification as to historical origin does not provide us with a classification as to function.

23 See White, 1945, for an elucidation of the "evolution versus diffusion" issue.

CONCLUSIONS

It is not my aim to contend that the Prophet Dance of the Northwest *is* a response to deprivation resulting from changes imposed by contact. Rather, I wish to point out that it *could* be, and that we are in no position to eliminate this possibility. But the principal points that I wish to make are methodological and theoretical. First, in considering the effects of contact, mediate as well as immediate contact must be examined, whether we are studying cults or other phenomena. These contacts may lead to transmission of information and to social change. Second, the number of types of cultural items received by a native group is not a reliable index to the amount of social change that may occur. Involvement in the fur trade at third hand and minor increments of tools may lead to transformations of a cultural order as profound as those resulting from some kinds of day-to-day direct contact. Last, total cultural collapse is but one special case of the more general phenomenon of deprivation, which includes the loss by a group of previously satisfactory conditions, the exposure of a group to new wants, and the imposition of loss of status, goods and the like on a portion of a total population, part of which may indeed experience new advantages. These three considerations appear to me of very general importance for the analysis of nativistic movements, cargo cults, and other cult activities among native groups.

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COMMENT ON ABERLE'S THESIS OF DEPRIVATION

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IN HIS LETTER of transmittal to the Editor Dr Aberle suggested that those whose views he examines in the preceding article in this issue might care to comment.

We would like to raise questions on two essential points: whether the Prophet Dance had its origin in a state of deprivation, and the validity of the extended meanings given the word "deprivation."

Since most of Aberle's suggestions, in so far as they pertain to the origins of the Prophet Dance are based on surmise, the question we raise here is: How far do the known facts concerning the dance support his thesis of its origin in deprivations? Aberle argues that because deprivations were present in its later stages, they must have existed—or at least, could have existed—at its beginning. Does this represent *a priori* reasoning in terms of a general theory of the origin of revivalistic cults which he feels must be substantiated?

His thesis may be stated in this way: Cults *may* be caused by deprivation; deprivation *may* be caused by contact; contact *may* be mediate as well as immediate; that in this instance there *could* have been mediate contact; hence there *could* have been deprivation, which *could* have given rise to the Prophet Dance. While, to be sure, his argument is put in contingent form, the net effect of his article is that deprivation *must* be the explanation.

Since some of his criticisms are directed to the approach in the original studies, we should make it clear that Spier and Suttles were concerned with tracing the forms and occurrences of the Prophet Dance and its derivatives, among them the Ghost Dance of 1890 which, long held to be a spontaneous outgrowth of deprivations among the Plains Indians, had its antecedents long before in the Plateau. Until this history is in hand it is futile to discuss or conjecture the social circumstances and mental condition of adherents.

No one will deny that changes in culture—whether due to deprivation, frustration, dislocation, or, it should be noted, to enrichment—may be involved in the origin and growth of cults. Certainly there is no question that a variety of tensions, disenchantments, and deprivations moulded the *later* course of the Prophet Dance. But there is no positive evidence that this cult had its *origin* in deprivation. To assert that the absence of such evidence in the early records compels us to infer that it was nevertheless present—because theory demands it—is reasoning we

cannot follow. Aberle at times seems close to asserting this. To state merely that deprivation *could* have been present—which is all he actually says—is undeniable. But this is scarcely helpful since it remains no more than an assumption until specific evidence is adduced.

The evidence shows that the Prophet Dance is at least as old as the late 18th century in the Plateau. At that time there were no Europeans there nor even very near. The supposition that about 1800 or earlier, the Plateau tribes could have been affected in some ill-defined way by Whites at a remote distance will have to be demonstrated before it is acceptable. Without proof this is no more than speculation. The mere knowledge that Whites were on the Missouri or touching at the Coast (if indeed there was such knowledge) is hardly warrant for assuming that tensions were rife. During the first decade of the 19th century there were a half-dozen small trading posts in this vast area, with their handful of engagés, while a scattering of trapping and exploring parties traversed parts of it. Suttles' outline of White contacts of this period on the Coast reveals how feeble a source this could have been for influencing the interior tribes. The historical and ethnographic records for the Plateau, which are fairly voluminous, do not suggest that there was interference with native life. As for epidemics in the Plateau, however disastrous they may have been (and Mooney's summary may be questioned), not one Plateau informant has ever cited them as occasion for renewal of cult activity. Prior to 1840 some trade goods did reach these Indians, but these provided an addition to ease of life, not an instrument of cultural dislocation. The major effect of White "contact at a distance" was a quite indirect stimulus which brought great enrichment in the southern sections by an inflow of Plains culture, itself stimulated by the acquisition of the horse. It is difficult to see in this early period any significant "contact at a distance" that disturbed the normal course of native life, and produced those tensions, desperation, and fear of impending disaster demanded by Aberle's theory of the origins of the Prophet Dance.

Parenthetically, it would seem that Aberle is in error in citing the Chipewyan case of 1830 as an analogue of the cargo cults: there is no indication of social distress, nor do we find a belief that goods would be supernaturally supplied, both fundamental elements in cargo cults. The Indians threatened drastic measures to secure more trade goods; they had lost nothing of their usual mode of life.

Aberle's concluding statement that he is not maintaining that the Prophet Dance *was* in origin a response to deprivation but *could have been* thus remains a bit of speculation unsupported by the facts. There is nothing to show that the cult was revivalistic in the sense of protest or rebellion against alien pressures until its later days: this appears most clearly in the Californian and Plains Ghost Dances,

both derived from it. After 1840 the relative immunity of the Plateau tribes changed: White settlements were established, though very sparse and scattered at first, and the wars of the 1850s were disruptive for some tribes. Only from the fourth or fifth decade could deprivations have begun to be of moment in motivating an erasure of the foreign elements and restoration of the old days.

Implicit in Aberle's argument is the proposition that cults of rejuvenation arise primarily in conditions of distress. Is there no other possibility? Aberle admits such a possibility but it would seem that he holds it merely theoretical. But there is no reason why, without deprivation, a cult may not originate and flourish for the sake of realizing the desire for the return of the beloved dead or for the return of an earlier day. Many of the ingredients of the formulated Prophet cult had a wide and deep occurrence in the cultures of the Northwest Coast and Plateau. Forms of the Orpheus myth, with its poignant failure to bring back the dead, were present there. From all these might have arisen a cult designed by intensity of effort to achieve what the myth proposes. Earth-renewal cults exist in other regions as well—from Cheyenne and Karok to the Australian aborigines—which accent replenishment of the earth or an opportunity to begin afresh with life renewed. On another scale life-renewal is a recurrent theme among tribes from Siberia to Labrador (first-salmon rites, care in the disposal of bones of bear, deer, and other renewal rites). Anxieties of a sort were presumably involved in most of these cases (though there is no suggestion of this for such a tribe as the Cheyenne) but these rites were not the consequence of cultural and social dislocation. It does not seem necessary to assume that distress is alone the prime causative agent.

The situation we envisage here—a cult most probably originating from purely native preoccupations, with revivalism as a later consequence of contact and distress—has also been proposed for the cargo cults of New Guinea in place of the earlier view that they sprang *de novo* from a state of disillusionment. "Melanesian religious beliefs [apart from the cults] often include apocalyptic ideas, and recent writers have pointed to other relationships between the cult beliefs and the indigenous religion" (K. E. Read).

We want to respond to some specific criticisms made in the early paragraphs of Aberle's article. Herskovits' statement is not to be regarded as self-contradictory. In rephrasing Herskovits' statement concerning the sources out of which the Ghost Dances of 1870 and 1890 arose, Aberle gives an interpretation that can be derived from what Herskovits actually stated—which Aberle quotes—only by distorting what is clearly meant. Even as quoted (out of context, we may say) what Herskovits wrote is: "What has been thought by many persons to have been a *purely* contra-acculturative phenomenon is shown actually to have *arisen* out of deep-

seated aboriginal patterns . . ." (italics ours). Aberle's ascription of "dimensional heterogeneity" would seem to be related to his tendency to look at the studies he criticizes in terms of an all-or-none dichotomy. Despite what he says, it has, for instance, been a long time since responsible anthropological theorists have placed evolution and diffusion in strict opposition. And was it ever anyone's position that proving diffusion disproves functional integration?

Nor is there confusion in Spier's statement. The 1870 and 1890 Ghost Dances, to repeat, did *not* spring full panoplied from conditions of distress; an older form was turned to nativistic and revivalistic account when acculturative conditions became intolerable. To show that the Plains tribes received the Ghost Dance from tribes to the west is not to deny that the Plains tribes accepted it because of their desperate situation in the face of White conquest. But to show that the Ghost Dance had antecedents in the Plateau Prophet Dance at a time before White contact is to disprove immediate contact as a cause of the origin of the movement, and, considering the date, *to throw doubt* on mediate contact as a factor in its earliest pre-1870 development.

Certainly at no point does Suttles neglect the influence of alien impact in the development of the Coast Salish derivatives: he is concerned primarily to point out the source of the marriage dance from a Prophet Dance antecedent. But he also depicts the development of new social forms as a reaction to missionary and perhaps other outside influences, both mediate and immediate.

Much of Aberle's case for deprivation as an originating circumstance hinges on his definition of deprivation, or rather, his several definitions. To this we can respond only briefly here. In using the term Aberle extends its meaning so as to make it nearly synonymous with any kind of frustration. Is it not wisest to keep this word to its original and most common meaning—a loss or debarment; here the loss of cultural and social integrity of an entire community or debarment from following the patterns of aboriginal life? This seems to be what is meant by his first type of deprivation.

His further analysis of related forms of mental stress is very suggestive and will certainly induce more careful inquiry in particular cases. Jealousy and desire for what other peoples have may well motivate a whole community; that there may be no way to secure these blessings for themselves might be called a debarment but there is no loss involved. As Aberle suggests, a shift in status might also be socially disruptive, involving loss and debarment for some individuals and for them a revulsion against the altered conditions phrased as a revivalistic cult. This in itself, however, would not necessarily lead all the members of the community to a nativistic movement; for them deprivation would have to be on a scale that

affected them all. But an enhancement, an enrichment, hardly accords with Aberle's definition of deprivation as a "negative discrepancy between expectation and actuality," that is, a lack or frustration. These three further situations all provide conditioning factors leading to cultural and social change, but that any or all would lead to group reaction against acculturative influences is problematical. Only confusion and indifference to particulars of social circumstances and mental conditions will result from subsuming them all under the single term "deprivation."

It is perhaps worth noting that in Africa the deprivations and disbarments inherent in the colonial situation have given rise to almost no nativistic movements looking toward the restoration of the past.

We suggest that the approach urged in Aberle's article represents little that is really new in anthropological science. Insofar as it repeats the well-known caution that a study should not ascribe a complex phenomenon to simple causes we are, it goes without saying, fully in agreement with him. But does he not reduce his own argument to a point where no one could possibly quarrel with it when he says, "It is not my aim to contend that the Prophet Dance of the Northwest *is* a reponse to deprivation resulting from changes imposed by contact. Rather, I wish to point out that it *could* be . . ."? Who, we may ask, has contended it couldn't?

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SOCIAL STRUCTURE AND SEX ANTAGONISM

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THE PROBLEM

THEORIES of social and cultural evolution have tended to deal with the quantifiable as units of study. In a quite crude way, anthropologists have spoken of the "cumulative" nature of culture, its tendency to expand in gross inventory of cultural items. Others have dealt on a more sophisticated level with man's growing control over the environment and sources of energy. And still others have oriented their interest toward the social system and an implicitly quantitative gradient of complexity, or structural differentiation. This, I believe, has had great methodological merit, for we have thereby avoided qualitative judgment and the intrusion of wholly subjective criteria of evolutionary level. But in so doing, anthropology has elected to deal with culture only in terms of mass of traits or with problems having a societal rather than symbolic locus. Little attention has been given to the evolution of the cultural symbols themselves, except in terms of methodologically untidy references to the emergence of scientific rationalism, of man's increasing grasp of the reality principle.

It is perhaps extremely doubtful that all or even the greater part of cultural symbolism can be encompassed by a general evolutionary theory, and this is certainly not my intent in this paper. I wish rather to discuss a limited aspect of symbolic behavior and to relate it through functional analysis to social structure, a realm in which evolutionary process is more readily discernible. The particular symbols to be discussed are those that are amenable to analysis in terms of Freudian theory; that is, cultural forms that are clearly projections of unconscious psychological materials and processes.

The notion that the psychic ontogeny of the individual recapitulates phylogenetic experience is well known in anthropology, though primarily through its notoriety. The student of the subject receives, however, the disturbing impression that although the theoretical explanation of recapitulation given by psychoanalysts is erroneous, it nonetheless has a strong element of empirical validity. Kroeber, for example, in his descriptive generalization upon the face of "progress" says:

> In summary, the quantitative expansion of the content of total human culture; the atrophy of magic based on psychopathology; the decline of infantile obsession with

1 The author acknowledges with gratitude the support of a Faculty Research Fellowship of the Social Science Research Council during the writing of this paper. The helpful comments and criticisms of L. Bryce Boyer, M.D., are also gratefully noted.

the outstanding physiological events of human life; and the persistent tendency of technology and science to grow accumulatively—these are the way in which progress may legitimately be considered a property or an attribute of culture.[2]

Kroeber attempts no explanation of his second and third criteria of progress, but there is nonetheless a clear implication in his statement that things that are normally repressed in higher civilizations are often institutionalized and formalized in primitive societies. Freud, of course, dealt with the same problem in *Totem and Taboo* and arrived at the conjecture that the early experiences of mankind are perpetuated by the "collective mind," a metapsychological concept analogous to the "racial memory" of Jung. Freud was aware that his hypothesis was untestable and he wrote of the problems involved in this view: "It must be admitted that these are grave difficulties; and any explanation that could avoid presumptions of such a kind would seem to be preferable."[3] It is the purpose of this paper to attempt such an explanation. As such, it is frankly exploratory.

The problem at hand is extremely broad and can, perhaps, be best approached on the level of system discourse. I choose, however, to make my point through analysis of the men's secret society among the Mundurucú Indians of central Brazil. The use of such an example has a certain merit inasmuch as similar organizations and their related symbolic complexes have been studied by psychoanalytically-oriented scholars and also because the widespread distribution of closely analogous phenomena makes comparative study possible. Men's secret societies in such diverse locales as Africa, Melanesia, Australia, and South America exhibit regularities of such a high order that they have long attracted the attention of anthropology. Webster considered them in terms of his overall scheme for the evolution of associations.[4] Lowie, on the other hand, thought the similarities between the cults to be so great that they could only be explained by diffusion.[5] It must be admitted that secret society culture was no doubt frequently communicated from group to group and may even have been brought to the New World by a migrant Palaeo-Mongoloid population. But this hardly explains the persistence of the complex or the bases of receptivity in the borrowing groups. And beyond this, secret societies are fundamental to the social structures of many primitive peoples and are not things that are simply copied and borrowed, like a pottery design. In short, some special explanation of a sufficient causal—and functional—kind is necessary to explain their wide distribution in the face of vast geographical discontinuities.

2 Kroeber, 1948, p. 304.
3 Freud, 1952, p. 158.
4 Webster, 1908.
5 Lowie, 1947, p. 313.

THE MUNDURUCÚ MEN'S CULT

The physical and social separation of the sexes among the Mundurucú is quite complete. The dwelling houses, of which there are usually three to five in each village, are the residences of the females and pre-pubescent boys. Each houses an extended family, the membership of which is determined by matrilocal residence; it is thus composed of a group of women related in the female line. These units are not, however, matrilineages in the formal sense of the term, and they have no legalistically identifiable corporate character. Formal descent is traced, rather, through the male line, for the Mundurucú have patrilineal clans, phratries, and moieties. The unilineal descent groups lack local nuclei due to the rule of residence, and their membership is scattered throughout the several Mundurucú villages.[6] The corporate character of these unilineal descent groups has, accordingly, become almost obliterated. The relative absence of such corporate units has had an interesting effect. The presence in many matrilocal societies of formally constituted matrilineages within which men exercise decision-making functions militates against the distant removal of a man from his natal household. The absence of such units in Mundurucú is correspondingly accompanied by absence of local endogamy. Villages are by no means exogamous, but most of their adult men come from other communities through marriage.

Those men who leave their native villages for residence in that of their wife are not tightly incorporated into a household unit, for all post-pubescent males reside in the village men's house. There, they sleep, eat, work and relax. Only the passage of time and the arrival of children establishes closer bonds with the household of the wife. For the most part, the life of the male is centered on the men's house, and each sex constitutes a focus of social interaction. Reinforcing this basic dichotomy, each sex is a self-consciously solidary unit as opposed to the other, and mutual antagonism is often displayed on ritual, and other, occasions. Opposition between the sexes as groups is, of course, complemented by the residence arrangement; the village males are outsiders and the females are natives; lines of sex antagonism are thus coincident with those between affinals.[7]

The social segregation of the sexes is continued within the division of labor. Men hunt, fish, and clear land for gardens. The women plant, harvest, process manioc flour and all other foods, and tend to the household chores. The only tasks in which men and women work together are the planting of manioc and the collec-

6 See Murphy, 1956.

7 This ideal form of opposition represents only a tendency, of course, for marriages between men and women of the same village are frequent. Also, the families of chiefs are patrilocal, and other patrilocal marriages occur sporadically.

tion of fish killed with timbó poison. Communal work is common among the Mundurucú, but it rarely transcends sex lines. The men of a village often hunt as a group, and all the women of the community usually help each other in manioc processing. Thus, the sexes often operate as work groups, but their tasks are well-defined and do not overlap. Beyond the basic sexual division of labor there is little further differentiation of work; the hereditary shamans and chiefs ply their specialties but they otherwise take full part in economic production.

Mundurucú culture is strongly male-oriented. The most highly valued body of traditional activity relates to warfare and hunting, and these pursuits are also the subject of the principal Mundurucú ceremonies. The woman is supposed to be docile and submissive, obedient and faithful to her husband. In actual practice, however, the separation of the sexes and the marginal participation of the men in the extended families of their wives inhibits the direct and personal exercise of authoritarian control of men over women.

There are, however, ritual techniques for the ordering of relations between the sexes that are an integral part of the total men's organization. Attached to each men's house is a small, completely enclosed chamber that houses the village's sacred musical instruments *(karökö)*. These consist of a set of three hollow wooden cylinders, into the open ends of which are inserted reeds. When the reed is blown, the cylinder emits a deep vibrant sound. The instruments are completely taboo to the sight of the women, and the doorway to the *karökö* chamber is arranged in the form of a baffle in order to prevent even a casual and inadvertent female glance. The instruments are believed to harbor spirits that protect the villagers from harm and aid in the maintenance and increase of the supply of game animals. In order to secure this benevolence, the supernaturals must be pleased by a daily offering of food and the frequent playing of the instruments. Through their control over the *karökö* the men thus play a vital part in the promotion of animal fertility and social well-being.

That the sacred musical instruments are secret to the women has further significance in the rationalization of sex roles and the validation of the superordinate position of the male. The necessity of secrecy is based upon the belief that the sex controlling the *karökö* thereby achieves dominance. This is seen most clearly in the myth of the discovery of the instruments. According to this tale, three women were walking in the forest when they heard music that seemed to emanate from a lagoon. They investigated but saw nothing except fish swimming in the waters. Each woman caught one of these fish, and it immediately turned into a *karökö*, giving a set of three. The women played these instruments daily until discovered by one

of the men, who induced them to bring their find to the village. They did so, but the power contained within the *karökö* enabled them to seize the men's roles and prerogatives. The women occupied the men's house, and the men lived in the dwellings. While the females did little but play the instruments the men had to make manioc flour, fetch water and firewood, and care for the children. Their ignominy was complete when the women visited the dwellings at night to force their attentions upon them ("Just as we do to them today," added one informant). But the women did not hunt and therefore could not make the ceremonial offerings of meat to the *karökö* spirits. The men, as the hunters, threatened to withhold the offerings of game, and thereby incur the displeasure of the spirits, unless the women yielded the instruments. The women were forced to submit and sex roles became as they are today. But the men must still guard the *karökö* from the women if they are to keep their dominance.

I was unable to obtain an actual case in which a woman spied upon the sacred instruments, but the Mundurucú men were firm in their insistence that any such violation would be immediately punished by dragging the offender into the underbrush and submitting her to gang rape by all the village men. Such offenses are no doubt rare, but the same punishment is meted out to women who, in other ways deviate markedly from sex-ascribed roles. These cases of gang rape usually concern women guilty of flagrant and aggressive promiscuity. Lest the impression be given that this is punishment for sin, puritanically defined, it should be made clear that most women indulge in an occasional pre- or extra-marital liaison. These pecadillos are the subject of gossip, but they are strictly private delicts. As such they are the proper concern of the offended husband, who usually attempts, with variable outcome, to beat his wife. On the other hand, when a woman is openly promiscuous and actually takes the initiative in sex relations, she is then manifesting behavior appropriate only to the male and thereby intrudes upon and threatens the masculine role. Her acts are thus a community concern, a public delict, and her punishment becomes the proper duty of all the village men.

Another occasion upon which gang rape is common has a peculiarly modern setting. Missionaries have established a boarding school among the Mundurucú and have gathered there some forty students ranging from about six to sixteen years of age. Parents are induced, usually against their will, to send their children there; the inmates generally make every effort to escape. Now when a boy flees the school, he has universal sympathy, but when an adolescent girl runs away, she is raped by the men of the first village near which she passes. Actually, there is considerable sympathy for her desire to leave the school, but none for the measures she has taken. By running away, she has placed herself beyond masculine protec-

tion, and she has also flouted male authority, albeit that of foreign missionaries whose objectives find limited support among the Mundurucú.

Gang rape occurs sporadically in many societies, including our own, but it seems bizarre in an institutionalized form. Actually, the Mundurucú are not at all unique, and we have numerous accounts of it in the ethnographic literature. Mead, for example, reports gang rape among the Omaha.[8] Although wanton women are stated to be the victims, her evidence suggests that these wantons are aggressive and selective in their love affairs, a clear parallel to the Mundurucú. The same author elsewhere states that the age mates of an Iatmul husband may "rape his recalcitrant wife into submission" at his request.[9] Another example is reported by Wagley, who observed the gang rape of a Tapirapé woman who refused to join the other females in manioc processing.[10] The woman in this case was unmarried and she was turned over to the village men for punishment by her brother. The action of the Tapirapé brother suggests that the deviation of a female constitutes a crisis that requires direct and immediate action regardless of bonds of kinship. The latter become eclipsed and secondary, and the maintenance of sex roles and sex groupings can thus be seen as a primary structural element in the society. This is evident in Mundurucú gang rapes. Not only are the exogamic restrictions connected with moieties and clans disregarded, but in at least one instance a true patrilateral parallel cousin took part in the assault.

INTERPRETATION AND ANALYSIS

The symbolic behavior discussed in this paper, however exotic, is nonetheless quite meaningful. Using a Freudian analysis of symbols, one of the most striking features of the whole complex is the form of the sacred musical instruments themselves.[11] Such objects, usually called flutes or trumpets in the literature, are quite commonly used for roughly the same purposes in men's secret societies in other groups. Functionally, they are equivalent to the more widely distributed bull roarer. Both emit deep, vibrant sounds and are used in some way to control the female portion of the population.[12] The bull roarer has usually been interpreted as a

8 Mead, 1932, pp. 91-92.

9 Mead, 1949, p. 52.

10 Charles Wagley, personal communication.

11 I am deducing from Freudian findings here, not trying to prove them. My argument obviously rests in part upon the validity of this psychology, and in part upon its own internal coherence and its success in explaining a body of data.

12 The noise is commonly explained to women and uninitiated males as the voice of spirits; this is not the case among the Mundurucú. Whatever the rationalization of the sound, the acoustical qualities show strong parallels. This may well be purely accidental, but it merits further consideration.

phallic symbol by students of Freudian inclination. I accept this view and would extend it to the more clearly phallic tubular wind instruments. The identification of the latter as a phallus depends upon much more than their external form, persuasive though this may be, for the Mundurucú men consciously believe their power and dominance to rest in their possession of the *karökö*. Moreover, the fertility and well-being aspect of the sacred instruments and their spirits gives the men a role in a normally female realm. It is instructive to compare these phases of the Mundurucú men's cult with that of the Mountain Arapesh:

> Among the Arapesh the *tamberan* is the embodiment, in some noise-making device, of the spirit of the male rituals in which the adult men assert their solidarity, reaffirm their masculinity, and produce growth and welfare for all the people.[13]

Thus far, I have stated that the secret instruments are phallic in form and, as the repositories of male power, in function. From this viewpoint, the men ritualize their role with the symbolic equivalent of the male organ. That this interpretation is not completely far-fetched can be seen in the ritual of rape, for here the men quite actively and vigorously maintain their position with the real penis, and not a surrogate flute. And as one informant quipped with no little humor, "We tame our women with the banana." Here is the expression in verbiage and social action of phallic sadism and the fantasy of the penis as a weapon and a source of power. It should be noted, lest the impression be given of grim genital retribution, that the Mundurucú men look upon these subjects with some hilarity. Gang rapes are carnival occasions that become the topic of endless anecdotes told in men's houses. And the men also refer to the vagina as "the crocodile's mouth," a marvelous bit of imagery that expresses perfectly the latent antagonism between the sexes, so patently a part of all vagina-dentata motifs. That these subjects are looked upon as humor does not negate their significance; humor, after all, it a fruitful source of otherwise inaccessible data.

The forms of these secret musical instruments, whether flutes or bull roarers, obviously do not frighten or cow the women, phallic though they may be, for the women never look upon them. Rather, they serve as ego-props for the men themselves, and reflect an insecurity, a threat, that is clear in the Mundurucú myth recounted above. This tale is a true social charter inasmuch as it explains and legitimizes sex role differentiation. But in common with the origin myths of men's cults in many other societies, it refers to a period when the women held sway over the men. It is noteworthy that sex role differences are not viewed as being neces-

13 Mead, 1940, p. 426. See also Bettleheim's (1954) essay on the institutionalization of womb-envy in puberty ceremonies.

sarily in the nature of things, given in biology and taken for granted, but depend rather upon a mythological self-assertion by the men. And the boundaries between the sexes must be constantly maintained through ritual and other more directly defensive measures against intrusions upon the male domain. The cultural evidence indicates that not only are the men insecure about their status, but there is fear that the women would attempt to arrogate their prerogatives, if allowed.

The cultural symbolism involved in the Mundurucú men's secret society may be seen as projections of the classic psychological themes of sex antagonism and ambivalent, perhaps incomplete, sex role identification. Sex antagonism is generally attributed in the psychoanalytic literature to the Oedipal situation and ambivalence toward the mother, thence through the mother-imago to females, in general. The symbol system manifests another motif of Oedipal origin, that of castration anxiety, or general fear of emasculation. This is most evident in the vagina-dentata theme, but it also underlies the theme that male tenure of masculinity, and its social concomitants, is not absolute. It is at this point that the organ-envy component of sex antagonism becomes significant, for the cultural data imply a recognition by the men that the women desire their power.

Now if these are indeed the sufficient psychological bases of the symbolic accoutrements of the men's cult among the Mundurucú, and in other societies, we are faced with a number of difficult problems. First, the psychological factors outlined above are well-nigh universal in human ontogeny, if not completely so, whatever the culture. Also, is not sex antagonism, latent or manifest, equally a part of all human experience? Is it not ontogenetically functional to successful sex differentiation? Why then if sex differentiation, both social and psychological, is not completely determined by heredity and if females threaten males in most, if not all, societies, are we not all swinging bull-roarers or playing sacred flutes? After all, any explanation of the men's cult that pretends to generality must tell us not only why it is present in some societies, but why it is absent in most.

I offer the hypothesis that though the psychological bases for these projective symbols approach universality, their integration into certain cultural systems is a function of social structure. A cursory survey of the ethnographic literature shows the men's cult to occur most commonly in societies that show rough typological congruence to the Mundurucú, and thus supports my previous analysis of the place of the organization in the latter group. This type society is simple and relatively undifferentiated. In such groups, the primary line of sex differentiation is not criss-crossed or blurred by multiple modes of role designation. Moreover, the division of labor is usually minimal—that is, between men and women. But we do not find the men's organization among the structurally most simple people in

the world, such as the Polar Eskimo and Shoshone. It tends, rather, to occur in societies having larger local groups and in which economic coöperation is a stable and daily affair. In such a setting, members of each sex are in daily association and engage in common pursuits, frequently in coöperation, to the virtual exclusion of the other sex.[14] In short, the sexes are true social groups, and the locus of social control over the women is vested in the males as a social entity. Sexual solidarity is too strong in systems of this type for role regulation and enforcement to be otherwise. Institutionalized sex antagonism in these societies is thus not just functional to the sex differentiation of the individual, does not just provide a form of abreaction of primary experiences, but it also maintains the internal solidarity and external boundaries of the sex-ascribed groups through opposition. It is because of the fundamental importance of sex-role differentiation to the structure of these societies that we find that other modes of role ascription are suspended on those occasions when social control over women is forcibly exercised.

Related to this opposition between the sexes, there is a tendency also for men's organizations of the type under discussion to be associated with unilocal residence. Unilocality, especially when accompanied by frequent marriage outside the local group, as in Mundurucú, keeps consanguines of one sex together while sending those of the opposite away. Members of the opposite sex thus tend to be outsiders and affines, and the forms of opposition common to the latter relationships complement sex antagonism. Moreover, in such systems, overt acts of hostility against women who are also close kin are less likely to occur; the role conflict inherent in our Mundurucú and Tapirapé examples is thus avoidable. Finally, that men's secret societies and their concomitants appear more frequently among patrilocal societies than in matrilocal ones may well derive from the fact that the latter tend toward local endogamy. Coextensive segregation along lines of both kinship and sex is thus inhibited.

CONCLUSION

I have explored in this paper a theoretical framework for the study of the evolution of symbols: a framework that recognizes some empirical validity in the Freudian recapitulation theory but which attempts a rather different explanation. My general thesis is quite simple. It states that the stuff of the unconscious tends to be expressed in cultural symbols where it serves some function in terms of social structure. And insofar as the latter can be studied in evolutionary terms, so also can we see certain projective institutions in an evolutionary perspective.

It will be noted that I have not attempted to relate the symbolism of the

14 Lowie (1947, p. 310) recognized the significance of the division of labor in producing what he called "sex moieties," although he did not systematically pursue this lead.

Mundurucú, or any other, men's cult to their specific techniques of child-rearing or to their modal personality types. Roheim, for example, does this in his attribution of the Australian men's cult to an aggravated Oedipal situation.[15] The sufficient psychological bases of these phenomena are too general in all cultures for such particularistic considerations to be relevant. It is this very similarity of human psychic development that accounts for the close parallels between men's secret societies throughout the world. If we are to seek efficient causes for the institutionalization of this unconscious material, however, we must look to social structure. That we Americans do not have bull-roarers and so forth could hardly be due to absence of the Oedipal experience and mutual organ-envy. We have them with a vengeance. The symbolic behavior characteristic of the men's cult is absent because our society is not structured along the simple lines that would make such rites functional. If our primitive contemporaries use techniques that seem to derive from primal experiences, it is because they are grappling with primal problems.

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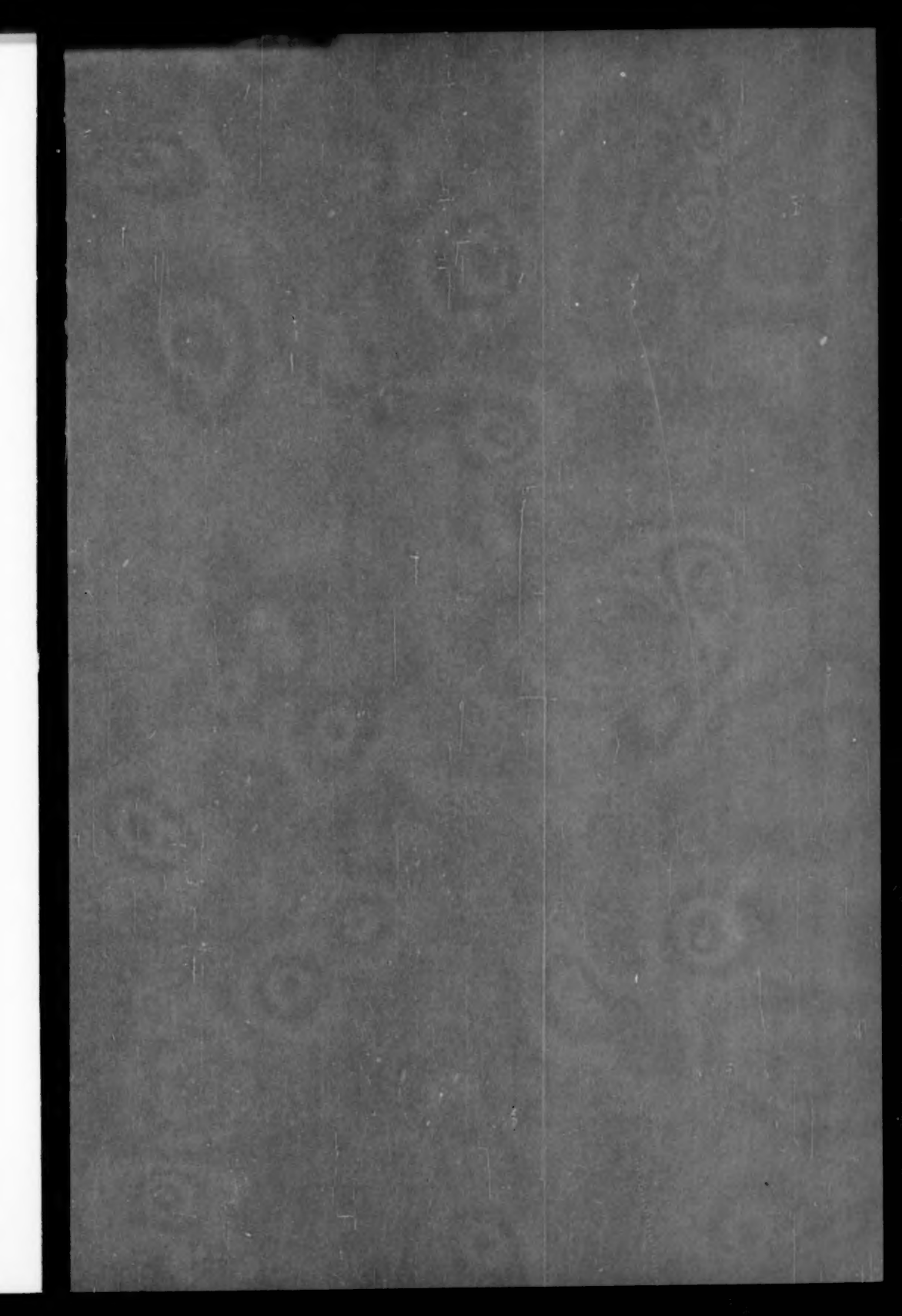
15 Roheim, 1934, p. 165.



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